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LES PHÉNOMÈNES FONDAMENTAUX DANS L'APPRENTISSAGE

(Résumé)

On arrive à la conclusion qu'il y a deux conditions absolument nécessaires à l'apprentissage, lesquelles sont donc les facteurs fondamentaux dans la formation des habitudes: *les réponses non conditionnelles* (ou conditionnelles bien établies) et *le temps*. Il faut que le facteur temps soit impliqué de sorte qu'une certaine quantité de stimulation (fréquence) ait lieu et que les facteurs qui modifient l'action neurale puissent opérer. D'ailleurs, il ne faut pas que la durée du temps (récence) qui sépare les deux stimuli ou réponses à lier soit trop longue pour l'étendue de l'association en arrière ou en avant. Puisque l'apprentissage dépend de la rétention des effets de l'exercice et puisque la rétention est une fonction du temps, la récence relative peut aussi influencer l'apprentissage par son rapport à la rétention. L'efficacité de la réponse non conditionnelle dans l'apprentissage semble dépendre fondamentalement des gradients antécédentes et subséquentes qui l'accompagnent. *L'apprentissage dans son caractère fondamental dépend donc de l'accomplissement de certaines conditions du temps dans les relations entre les stimuli et de la présence des réponses non conditionnelles ou des réponses conditionnelles bien établies avec leurs gradients.*

HUNTER

DIE WESENTLICHEN VORGÄNGE BEIM LERNEN

(Referat)

Wir kommen zu den Schluss, dass es zwei Bedingungen giebt, die notwendigerweise vorhanden sein müssen, wenn Lernen stattfinden soll,—Bedingungen die also die wesentlichen Bestandteile der Bildung von Gewohnheiten [habit formation] darstellen: *unbedingte* (oder fest etablierte bedingte) *Reaktionen* und *Zeit*. Der Zeitfaktor muss in einer solchen Form in Anspruch genommen werden, dass eine gewisse Quantität der Reizung [amount of stimulation] (Frequenz) stattfinden kann und dass die Einwirkungen, welche die Nerventätigkeit modifizieren, für ihre Einwirkung Gelegenheit finden können. Ferner darf der Zeitabstand (Neuheit) [recency], zwischen den zwei Reizen oder Reaktionen, die verbunden werden sollen, nicht so gross sein, dass er die Ausbreitung der Assoziation rückwärts oder vorwärts hemmt. Da das Lernen von der Beibehaltung [retention] der Einwirkungen der Übung [practice effects] abhängt, und da die Beibehaltung eine Zeitfunktion darstellt, kann die relative Neuheit das Lernen auch durch ihre Beziehung zu der Beibehaltung beeinflussen. Die Wirksamkeit der unbedingten Reaktion beim Lernen scheint grundsätzlich von den vorhergehenden und nachkommenden Gradienten, welche sie begleiten abzuhängen. *Das Lernen hängt also in ihrer wesentlichen Beschaffenheit von der Erfüllung gewisser zeitlicher Bedingungen in den Beziehungen unter den Reizen und von dem Bestehen unbedingter oder fest etablierter bedingter Reaktionen mit ihren Gradienten ab.*

HUNTER

VECTORS, COGNITIVE PROCESSES, AND MR. TOLMAN'S CRITICISM*

From Stanford University

KURT LEWIN

In his book, *Purposive Behavior in Animals and Men,* Tolman (12) treats many problems which are closely connected with our investigations. It is a great pleasure for me to find that the experiments with animals made by his collaborators (6) so nearly coincide with our experiments and theories.

Recently he made some criticisms (13) which touch upon a basic concept of my theory. These objections seem, in part, to arise from misunderstandings which may be easy to clear up but, at the same time, concern questions relating to the most difficult and fundamental problems of any dynamical psychological theory.

I. ANALYSIS, "MECHANISM," AND MYSTICISM

First as to the goal of our theories: It is an opinion as false as it is frequent (13, p. 4), that Gestalt-theory is against analysis. It may be true that in the early phase of this movement one can occasionally find such a tendency. But, today, there can hardly be any doubt that we try to analyze the dynamics of psychobiological events as penetratingly and as exactly as possible. I should say that one of the general differences between Tolman's theory and mine is that at several points my analysis goes farther than his. But surely we must discuss what one means by analysis.

One of the first steps of analysis must be to find out whether one is dealing with strong or weak dynamical wholes, with parts of wholes, or with sums. In this point I am sure I agree with Mr. Tolman. At other points we seem to have quite different ideas about analysis.

What is the task of analysis? What should be the aim especially of a dynamical analysis? In my opinion the goal of *analysis* is to *characterize the facts* so that one can *strictly derive the behavior,* or any other kind of psychological event, in a logical way, or (to say

*Accepted for publication by Carl Murchison of the Editorial Board.

the same from another point of view) to be able to *predict* actual occurrences. The theoretical and practical goal of analysis is reached when such derivation is possible and then the task is completed.

For such an "explanation" or prediction of events, we must know in psychology just as in every science

1. the general laws of the particular events
2. the momentary constitution of the whole situation.

In order to analyze the situation, one must characterize the situation, that is, the psychological environment and the psychological person, through concepts that are logically, clearly, and completely enough defined to allow logical derivations and which can be used as constants in place of the variable values of the equations which characterize the general laws. It is this task of prediction or derivation which will compel psychology to use more and more concepts of mathematical structure.

The *cause* of the events is the *relationship between the parts of the situation as dynamical facts,* and a complete characterization of these dynamical facts would be a complete analysis. I believe, therefore, that psychologists must learn not to ask about causes which are "more" than the relations between these dynamical facts.

Speaking about the "direction and distance relations on the one hand and demand-value on the other," Tolman says, "Their causes are *cognitive* and *motivational* processes *behind* the field and these causes are not to be confused with these resultant features of the field themselves" (13, p. 5).

Concerning this question, one must emphasize the following: If we represent the whole psychobiological situation, we have to deal with a psychobiological environment and with a psychobiological person in his environment. It is a very important fact that changes in the psychobiological environment are closely related to changes within the person. Both Tolman and I have emphasized this fact from the beginning. My collaborators have made a group of experiments especially in connection with this problem and formulated a group of laws (7, 14). So I can understand that a change in the state or the structure of the person can cause a change in the demand-value of an object in the psychobiological environment and thus be called a motivating process.[1] The changing of one part of the field,

[1]I personally do not like to use the term "motivation" because, just as the "will," it is too nearly connected with a non-biological group of heterogeneous theories and is not a very clear concept.

namely, the demand-value, is then the effect of a change of another part of the whole field, namely, the person. But I cannot see any other but a mystical meaning to find a motivational process "behind" the field.[2]

One example of his own analysis may show what Tolman means by his statement that I do not analyze enough.

Tolman remarks (13, p. 8), speaking about my concept of vectors, that I do "not offer any mechanism for this greater inability to do the indirect route as the pull is increased other than again to imply the solely perpendicular action of the vectors." To a special problem concerning this I will return later. First, as to the general problem: Indeed, I do not offer such a "*mechanism*," and I believe that the possibility of deriving some of the behavior of a person in an exact way without presupposing a special mechanism is a fruitful characteristic of my theory. The fruitfulness of the concept of gravitation in physics at the time of Newton was not lessened by the fact of not knowing the mechanism which creates this queer property of bodies to attract each other, and it would not have been reasonable to wait to apply this concept until one had found that such a mechanism does not exist. The sentence of Newton, "hypothesis non fingo" means just this, that he tried only to characterize facts and the relation between them in such a way that one can derive the events. I believe it is at the present time more important for psychobiology to find concepts which permit the exact derivation of the behavior of living beings from the psychobiological environment and from the structure of this living being than to continue to imagine one of the numerous mechanisms which could possibly account for the behavior. Indeed, we do not know whether it will be necessary at all to presuppose a real "mechanism."

Surely, also, our theory regards the organism as a unit only as a first approximation and only in regard to special actions and situations. I have emphasized from the beginning (10, p. 29) that the human being is not an unstructured extremely strong dynamical whole (starke Gestalt). I have emphasized that there are parts which for different persons and for different ages are separated in

[2]The tendency to search for the cause "behind" the field and not to see the cause in the dynamical facts and their relations themselves is somewhat similar to the kind of thinking that was done in the earliest phase of the Gestalt-theory (i.e., Ehrenfels), such as making the statement that the whole is "more" than the sum of its parts, namely, the sum of the parts plus the Gestaltqualität.

different degrees, and quite a group of our experiments (1, 3, 7, 11) try to determine this inner structure of the person. But let us return to Tolman's example.

Tolman himself offered such a mechanism in order to explain the effects of the strengths of the demand-value, namely, the "psychoanalytical concept of 'regression,'" (13, p. 9). This regression brings the child "to ignoring the barred character" of the direct route and causes it to revert "to the cognitive level of a younger child" (13, p. 9) which cannot find the roundabout route. It is possible to use the term "regression" to describe behavior under such conditions as being to some degree similar to the behavior of a younger child. But I do not find that we know the mechanism of regression. The concept "regression" seems to me not to be preferable to the concept "vector." Whereas the vector is logically clear and completely defined so that one can prove whether or not the consequences actually take place, regression has only been vaguely and incompletely defined. Only if one can characterize regression dynamically and can derive in a concrete manner why under affective conditions regression occurs, recourse to this concept would have value as an analysis or explanation. [In the work of Dembo (3) one can find the dynamical nature, the conditions, and the effect of the regressional process explained to a certain degree.]

Surely one must go "behind" the *phenomenological* description and represent the field as a group of dynamically or *conditional-genetic* defined facts. I have often emphasized how important this transition is. But this transition is the only way "behind." *It has no scientific value to go behind the dynamical field to some entities which are not parts of this field, whatever these entities may be, and whether one gives them philosophical or physiological names.*[3] For theoretically one can explain the event from the dynamical effects of the fields using the general laws as deriving principles, and practically one can predict what will occur.

II. The Definition of Valence and the "Cognitive Processes"

I am glad to agree with Tolman when he emphasizes the identity of some of his basic concepts with certain of mine. His concept

[3]This does not mean that "physiological facts" are not important. If one cares to distinguish between "physiological" and "psychological" dynamical facts, both must have a place in the representation of the whole field. For only such facts can affect each other which have a place in the same field.

of "demand-value" especially is an excellent expression for "Aufforderungscharakter," which has been translated as "valence" (9, p. 101). The definition for valence (10, p. 60; 9, p. 102), the distinction between positive and negative valence, the use of the tendency of objective action in a special direction as a criterion for the valence, indicate that this term and the "demand-value" are names for the same concept. I included in my definition that valence may vary greatly in strength of attraction: It may be impossible for the subject to withstand it, or it may have the character of a command or only the strength of a request or less. I think that Tolman's demand-value also includes the possibility of varying strengths.

Tolman sees a difference between demand-value and valence in this point: He believes that I, in using the term vector, correlate valence with "cognitive processes." Here, indeed, is a real misunderstanding. My definition correlates valence with actions, on the one hand, with needs, on the other. Also in the application of this term in my various papers and those of my collaborators I cannot see any case in which we use this concept in the meaning to which Tolman refers. I agree with Tolman in his report (13, pp. 5f.) of Blodgett's experiment (2) in which the rats become acquainted with the distribution and direction of the field even when no important demand-value is present. Surely a connection between valence and "cognitive processes" occurs in so far as the presence of certain valences can have an important influence on the direction and type of "cognitive processes." If, for instance, the valence is too strong, it can affect the cognitive processes.

The reason for this misunderstanding may be connected with two points. First, the field about which I am speaking is psychobiologically and not physically defined; secondly, the term vector seems to have been misinterpreted.

III. The Physical and the Psychobiological Field

In various places I have emphasized the point that to understand the behavior of man and animals one must describe the situation, not from the point of view of physics, but from that of psychobiology. A child of two months, for instance, and an adult may be in exactly the same physical environment, whereas the biological field in which each exists and which governs their behavior would be very different. It seems that Tolman agrees with this basic concept, defining "the environmental milieu (in so far as the organism is sensitive

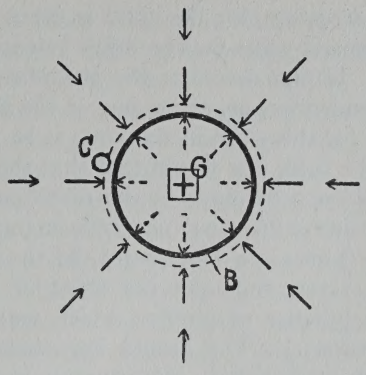

FIGURE 1

———→ driving force corresponding to the goal (G)
- - - -→restraining force corresponding to the barrier (B)
- - - - -line of equilibrium

to it)" (13, p. 4). Nevertheless it might be appropriate to speak about the work in which this basic idea seems not always to have been taken into account.

Hall and Ballachey (6), pupils of Tolman, attempt to bring the rat into a situation similar to one I used in one of my experiments with children: A positive valence is given in the center of a circular barrier. I made some special statements about the direction of the restless movements in such a situation. The investigation of Hall and Ballachey was intended, above all, to be "a contribution to method." I enjoy very much the progress from research work which uses mazes that are, in the dynamical point of view, above all resistent forces, to experiments with free-field situations.[4] I am pleased that our results, on the whole, seem to be confirmed. Nevertheless, particularly for work which has methodological goals, one must emphasize certain points.

In the experiments of Hall and Ballachey the rats were placed in a box in the center of which was some food surrounded by a circular barrier. The behavior of these rats was compared with that of other rats in a situation where food was not present. From a *physical* point of view one may say that the food with the barrier around it creates for the rat the same situation as the toy with the barrier around it for the child. But has the situation, from the psychobiological point of view, the same structure?

[4]Compare also the work of Dembo (4).

The dynamical structure for the child is given in Figure 1. The goal (G) has a demand-value and no other valences are at this time relatively strong. If one describes the force-field, that shows the directions of the tendencies, or, as we say, of the psychological forces which would exist for this child in this state if he were placed at the various points in the field, one would find that the child would have at any point a tendency to move in the direction of G. In other words, we are dealing with what one calls in mathematics a "central field." (The forces are represented, in the figure, by arrows. The head of the arrow indicates the point of application in accordance with the practice of physics, which we did not follow in our earlier publications.) The second important fact which characterizes the dynamical factors in this situation is that a barrier (B) of a specific form prevents the child from approaching G more closely than a definite distance.

My personal experience with rats is not extensive enough to permit me to make very specific statements. Nevertheless, I am sure that the first time one brings a rat into a box, the dynamical structure of the field will be very different from the structure as described above. There are differences, not only in the "cognitive structure," that is, in the direction, the distance, and the kind of psychologically existent objects, but also in the valences. As I understand it, the box was too large to permit the rat, coming in from one corner, to get immediately a clear survey of the situation. The wall (W) nearby may have been clear enough, but farther away the box probably was not very clearly perceived. It is a question of the absolute distances and the intelligence of the individual rat (R) whether or not the food (G), its properties and position, and the barrier (B) were also a part of the vague region of the field (Figure 2).

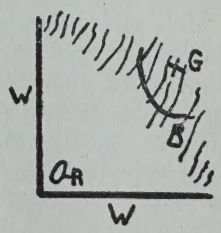

FIGURE 2

But even if the position of the food were perfectly clear, the dynamical structure would differ greatly from the experiment on the child. The rat was brought into an unfamiliar box. Wiehe has, during the last two years, carried out a group of experiments in which a child was brought into a strange room. We know what strong forces are created in such a new situation. Not infrequently a *strange* room creates dynamically such *high pressure* for the child that he is unable to move. If the pressure is not so great the child tends to move into a field in which it may feel at home, perhaps close to the mother. On the other hand, a strange object or strange person has very often certain *attractive* characteristics, and eventually, after the strangeness wears off a little, the tendency towards this person or object usually will be greater than the tendency away. A conflict situation thus occurs which creates some overactive behavior until the child becomes accustomed to the situation.

We know that the behavior of rats in a strange room varies greatly from that in one with which they are familiar. They like first to go along the walls. (Figure 2 in the work of Hall and Ballachey shows an example of this.) I do not know whether the reason for this typical behavior is a negative valence of the strange space in the center of the box, or a positive valence of the wall, or perhaps both together. (It may even be that there exists a tendency for the rat to explore the possibilities in the new situation.) It is

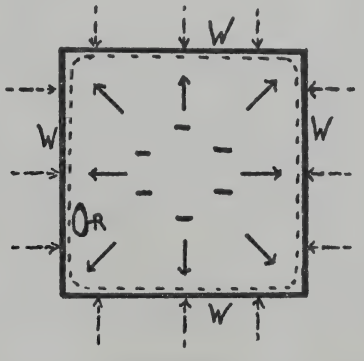

FIGURE 3

———→ driving force corresponding to the negative valence of the strange room
- - - -→restraining force corresponding to the wall (*W*)
- - - - -line of equilibrium

possible from our basic concepts to derive the behavior of the rat even if the wall itself has not a positive valence and only the strange space a negative valence. In this case, a force-field would exist approximately represented by Figure 3.

If only these driving forces exist the rat must stay without moving in a corner near the wall. But in such a situation a *tension* exists which creates restless movements (9, p. 114). In this situation the *restless* movements take the form of running along the wall, because this path allows the rat to move without going in a direction contrary to the field-forces; in other words, this is a line of equilibrium (see p. 327).

To determine whether in reality only this negative valence exists or also the positive valence of the wall one would have to develop special experiments. At any rate, in the beginning of this situation, a group of important demand valences exists, which can give the whole situation a fundamentally different structure from the situation of our child experiments.

According to Figure 2 of Hall and Ballachey, it would seem that at the end of the second trial the rat was familiar enough with the space so that the food in the center had such a relatively high valence that the rat moved in the direction of the food and circled around it. It may be that at this time the dynamical structure of the situation for the rat was nearly the same as for our child.

In the later trials, for instance in the fourth trial (Figure 3 of Hall and Ballachey), this period seemed to have a longer duration. Nevertheless, one cannot suppose that the more one repeats this experiment, the more the similarity of the two situations will increase. For, if the rat repeats the trial without success, the barrier and the field within will acquire some negative valence because of the failure (Figure 4*a*), or it will lose its positive valence[5] (Figure 4*b*) so that the dynamical situation will be changed and cannot be described as a central field consisting of a positive valence within a circular barrier.

The permanent constitution of the field after many repetitions seems to be similar to the field at the end of the first experiment with

[5]The latter generally occurs if the subject loses his hope after a long trial. The cause for this seems to be that the goal and his position behind the barrier, which at first are distinct facts psychologically, at last become fused to such a degree that only one unique fact exists; something unattainable, which as such is quite neutral.

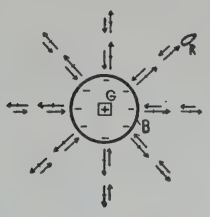

FIGURE 4*a*

⟶ driving force corresponding to the positive valence of *G*
⟶ driving force corresponding to the negative valence of the barrier

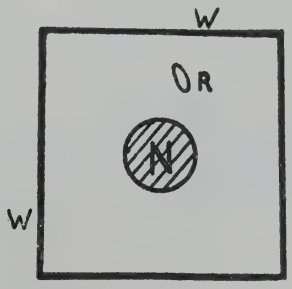

FIGURE 4*b*

N=relatively neutral zone (somewhat unattainable)
W=wall

the child. After the child has tried for some time to reach the goal without success, the region behind the barrier and the barrier acquire increasing negative valence which at last puts the child out of the field. Sometimes one can observe this gradual increase of the negative valence in the form of restless movements. In our case the restless movements occur along the line of equilibrium. As long as no negative valence exists or if it is very weak, this line of equilibrium is along the barrier (in Figure 1 represented by the dotted line).[6] If the negative valence increases, the line of equilibrium will be at a greater distance from the barrier (Figure 5).[7]

[6]In the situation for the rat represented in Figure 3, the line of equilibrium (dotted line) is along the outer wall of the box.

[7]For this result it is essential that the forces related to the negative valence decrease more quickly with distance than the forces related to the positive valence, but about this fact I cannot speak in detail here.

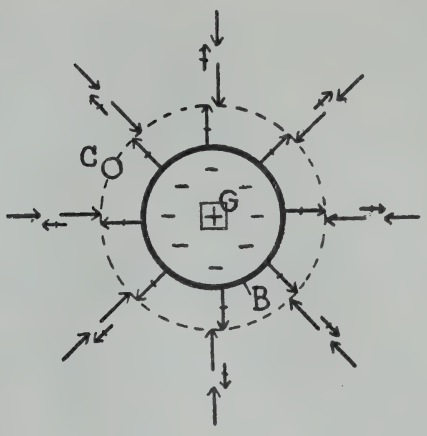

FIGURE 5

——→ driving force corresponding to the positive valence of *G*
—⊣→ driving force corresponding to the negative valence of the barrier
- - - - line of equilibrium

Indeed, one can in some of my films observe how the circle of the restless movements becomes greater.

It is thus important, from a methodological point of view, to emphasize the fact that *the only situations which should be grouped for statistical treatment are those which have for the individual rats or for the individual children the same psychobiological structure and only for such period of time as this structure exists.*. It is necessary to observe not only the physical but the psychobiological structure. Furthermore, one must avoid the mistake which often occurred in the earlier period of perception-psychology, namely, to consider only the small region of the whole situation which the experimenter had first in mind. It is not important how the experimenter sees the situation, but what kind of situation exists for the rat or for the child, and one must in every case describe the structure of this whole situation.

IV. Forces, Vectors, and Topology

The question of cause and effect has two meanings:

1. What will be the behavior of a given person in a given situa-

tion? (For instance, what is the effect of a particular distribution of valences and barriers on behavior?)

2. Why has the situation this structure and the person this state? (The question of cause has in this second case, therefore, a more historical meaning.)

The examples of which Mr. Tolman speaks, discussing the vector, pertain to the first question and particularly to what I call the "locomotion" of the person. In order to derive the locomotion of the person in a given field we use the concept of force in a way which includes a strict coordination between force and locomotion, on the one hand, and valences or barriers and forces, on the other hand.

Tolman has not, so far as I see, criticized the use of the concept of force. Indeed, it seems impossible to make any dynamical theory of psychobiological events without using this concept. The associationists made use of the concept of "tendency," implying force without expressing it. Any theory using the concept of "equilibrium" implies the idea of forces (since equilibrium is nothing other than a special case of distribution of forces). Even Tolman utilizes tendency toward or away from a demand-value with the same implication. In fact, the idea of valence or demand-value is worthless, if one does not coordinate with this idea in a strict manner the concept of force or tendency to locomotion and *define the force* through the fact that an observable *real locomotion* in its direction does occur if no other forces paralyze or change its effect, and that, whenever a real locomotion occurs, a force or a resultant of forces must exist in its direction.

A force has three properties: direction, strength, and point of application. To represent the first two properties I use the concept *vector* which seems to Tolman to be "bad" and to create "unclear hodge-podge." I believe that this concept is analytically as clear as possible and that only a misinterpretation, which I hope to be able to clear up, is creating this "hodge-podge." Nevertheless, Tolman touches upon certain very fundamental objective difficulties which will only gradually be cleared up and in which Tolman's cooperation in experimentation with animals seems to be of great importance.

The vector is a mathematical concept to be compared with the number concept. From the standpoint of scientific theory, one has as much right to use this concept to represent psychological facts as to use any other mathematical concept. Mathematicians distinguish between "scalars" which can be represented by one number,

and vectors which are values possessing direction and strength. (For an algebraic representation of a vector one must use two numbers.) Further description can be found in any mathematical textbook describing vector analysis.

Tolman is in error in believing that the vector is a concept of topology. *Topology* is a more fundamental mathematical discipline dealing above all with the possible connective relationships between all possible "spacial" regions. This discipline is the basis of all kinds of geometry, but the concepts of directions and distances as well as the concept of vector go beyond purely topological facts. It would be possible to use topology in psychology even if the concepts of vector, direction, or distance were not applicable in psychology.

The problem of whether or not the concept of *vector* can be used in psychology reduces itself to the question of whether or not one can distinguish in the psychological field different directions and different strengths. It does not at the moment seem necessary to discuss the question of *strength* even though there are considerable practical difficulties. Tolman's criticism is connected with the idea of *direction*. And, indeed, to give a general definition of direction, not in the physical but in the psychobiological field, is a task which appears problematic and which is surely not yet satisfactorily solved. I will return to this question later. Nevertheless, Tolman cannot criticize the use of this term because he himself makes use of direction as a basic concept in his definition of the environmental field. And, indeed, difficult as an exact definition of direction in a biological field is, the use of such a concept is unavoidable and in many cases very fruitful.

It may be added that one can use the vector concept, not only to describe some properties of forces, but also, for instance, to describe the direction of locomotion. I make use of the vector concept in the latter sense, also.

V. The "Perpendicularity"

Tolman, in speaking about vectors, frequently uses the word "perpendicular." He asks, for instance, "why does the vector always pull perpendicularly?" (13, p. 7), and says, "why a strong vector should have a relatively stronger perpendicularity than a weak vector is not made clear" (13, p 8). Sometimes it seems as though Tolman means perpendicular to the barrier, and again he seems to have other ideas in mind. At any rate, I do not understand this pre-

sentation of my concept. For in my representation of the dynamical structure of the field, the vector characterizes the direction of the force itself, that is, the direction in which locomotion would occur if no other forces were present. If, for instance, only one positive valence governs the field, then the vector has the direction from the position of the person to the position of the positive valence. I do not see where to bring into this basic conception the idea of perpendicularity and fear that the particular subject of my first article in America (*Proc. 9th Int. Cong. Psychol.,* 1929, 286-288) was responsible for the misunderstanding. This article discussed the *restless* movements, that is, movements which are, as such, directed neither away from nor toward the goal, but are an expression of a general state of tension. Nevertheless, in some cases (e.g., Figures 1, 3, 5) one can determine the direction of these movements. But let us turn to the examples in Tolman's criticism.

VI. The Relation between Driving and Restraining Forces —The Effect of a Negative Valence

Discussing the effect of the strength of the valence on the possibility of actually reaching the goal, I drew attention to the paradoxical effect that sometimes (for example, if a small child is behind a U-formed barrier) the reaching of the goal will be more difficult if the valence is higher than a particular optimum. I used this example to explain that one can logically derive these facts from the basic statements of my theory without adding new theoretical suppositions. In such a situation the child, in order to reach the goal, must first go in a direction opposed to some degree to the direction of the goal. If the force toward the goal is increased, locomotion in the opposite direction must be more difficult. It is further important, as I have said (9, p. 105) that if the affective connection between the child and the goal is too strong "the relative detachment and inward 'retirement' from the valence which is so favorable to the perception of the whole situation and hence to the transformation (Umstrukturierung) of the total field, which occurs in the act of insight, is much more difficult."

Tolman's remark that I do not make clear why a stronger vector should have a relatively stronger perpendicularity than a weak vector is, I hope, answered by the above explanation: I can see no occasion to refer to perpendicularity in this instance. It is sufficient to recall our basic concept, namely, that if a positive valence increases, the force in the direction of the valence increases.

Tolman's own explanation is that the "older child *regresses* (at least temporarily) to a knowledge of the direct route only and to an ignoring of its barred character" (13, p. 9). About regression we have spoken above. Sometimes it may be possible to describe the behavior of the child as ignoring the barred character of the barrier. But what is the reason for this behavior? Tolman's answer is that "the barred character of the direct route drops out under the stress of such a too strong motivation" (13, p. 9). Tolman used a similar explanation for the difference between the younger and the older child: "The young child is sensitive to the direct route only and relatively insensitive to the barred character" (13, p. 7). And if the child *ignores the barred character* of the direct route, the indirect route has not as high a subordinate value as otherwise.

I do not think that one can compare in these cases the demand-value of the indirect with the demand-value of the direct route because for the young child, or if the valence is too strong, the "indirect route" does not exist at all, psychologically.[8] If this indirect route existed psychologically, the child would use this route. (We will return later to the general question under what conditions different routes exist psychologically.)

A second supposition of this derivation is not admissible, either. Tolman, in saying that the child under such conditions is "relatively insensitive to the barred character" of the barrier, seems to advocate the thesis that the *barred* character of a barrier decreases psychologically if the positive valence behind the barrier increases. I do not know on what experiences his statement is based. For the experiments of Fajans (5) with children from 6 months to 6 years show that just the opposite is true: *The barred character of the barrier increases if the valence behind the barrier increases.* I think it is easy to understand that one is more conscious of the barrier and the barrier is more disagreeable if the valence behind the barrier is very attractive. We have to deal here with the general fact that the strength of a *restraining force* (for instance, of the barrier) is a function of the strength of the driving force (for instance, of the goal behind the barrier). Therefore it seems impossible to use the explanation of Tolman.

This fact may be especially clear in certain cases of the "almost-

[8]Our theory refers to the degree of opposition between a direction of a motion and the direction toward the goal, and this fact exists psychologically under these conditions

situation." Tolman, in discussing my explanation of the unreasonable tendency to break out not seldom found among older prisoners or young delinquents who are about to reach freedom, is right in saying that these cases are similar to those above. But if the prisoner, after three years in prison, tries to break out two weeks before he is to be freed, he is acting not because he no longer knows how strong the barrier is, but because the positive valence behind the barrier is so strong that he tries to act in this direction despite his knowledge. Similarly, the reason for the bad behavior of children in institutions at such times is not due to the loss of knowledge of the barred character of the barrier. On the contrary, the closeness of the goal behind the barrier makes the child feel more intensely its barred character and the prison-like character of the institution (9, p. 109).

Tolman wonders about this last statement and interprets it to mean that I believe "that the almost-situation lends an independent negative valence to the intervening barrier in and of itself so that this barrier will come to be attacked directly and for its own sake" (13, p. 10). But here we have a misunderstanding. If my statement had been that the barrier in this case had an increasing negative valence, then the result would be just the opposite: for the negative valence is defined through the tendency to go away from it.

There are, in fact, cases in which the barrier or the region behind the barrier receives negative valence, for instance, when a child fails to reach his goal behind the barrier. As we have seen above, the increasing of the negative valence then leads in fact to locomotion away from the barrier. This is the reason for the temporary and, finally, for the permanent going out of the field.

It is true that even in the case of the "almost-situation" sometimes the barrier will be attacked. If one were to use the concept of disagreeableness, it sounds quite reasonable that the disagreeableness of a thing is the cause for attacking it. But if one uses clearly defined concepts like valence (demand-value) one is forced to be consistent. In these cases, therefore, one has to say that the negative valence must lead to the tendency to go away from the barrier.[9]

[9]The disagreeableness of something under certain conditions can create a tendency to destroy it. But in these cases one cannot derive the behavior from the negative valence alone, because such valence can create only actions away from it. There must be other forces, for instance, the desire to free the field of disagreeableness. Therefore one must under-

The reason for activity against the barrier can be only a positive valence behind the barrier or a negative valence behind the person. In our cases the increase of both is the reason. In most cases of real attacks on the barrier, one can characterize the action as a diffuse action in the direction of the goal as a result of the strength of this force and the affective state of the person, about which we have spoken before.

VII. The Concept of Path and Its "Subordinate Value"

Tolman, explaining the "almost-situation," employs also a second general principle which we must discuss in detail.

"My interpretation would be that, when the goal is just beyond the barrier, the ratio of the indirect-pointing segments of the necessary roundabout path to the direct-pointing segments of that path is relatively much greater than when the goal is considerably farther behind the barrier. But as I have already asserted, an indirect route always has, as such, less subordinate demand-value distributed to it than a direct route. Hence, when in the 'almost-situation,' the ratio of indirect to direct (segments) of the roundabout path becomes greater, the attractiveness of the path will become less" (13, p. 10).

Indeed, if the goal (G) is farther behind the barrier (B) (Figure 6), the direct segment (d) of the roundabout route will be comparatively greater, as in Figure 7, indirect segment (i) may be the same. Therefore: $\left(\frac{i}{d}\right)_7 > \left(\frac{i}{d}\right)_6$

Here we have a clear, mathematically defined statement. But there are many cases which are opposed to this theory. The barrier, for instance, may have the form of Figure 8, and $i_8 > i_7$; $(i+d)_8 = (i+d)_7$. In this case $\left(\frac{i}{d}\right)_8 > \left(\frac{i}{d}\right)_7$.

But in reality the roundabout route in Figure 8 is much easier than that in Figure 7. And so the theory cannot be true.

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stand the attack as a result of a conflict situation. A somewhat different interpretation is possible only if one coordinate to a negative valence not one force which acts upon the person, but two forces: one acting on the person, the other acting (in the opposite direction) upon the valence. It seems to me that many facts suggest this assumption. But without long explanation this theory at first seems so ridiculous that we will not speak about it here.

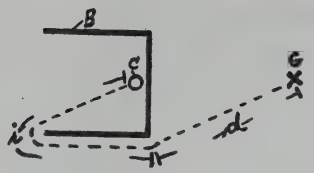

FIGURE 6

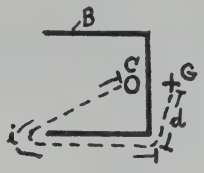

FIGURE 7

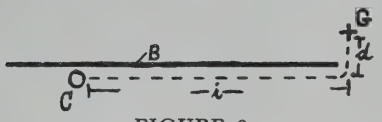

FIGURE 8

In general, it seems to me improbable that one can speak of "distribution of subordinate demand-values to different segments of a path." Are these subordinate values adding forces for the person to the forces which are the effect of the valence of the goal? One surely cannot use such a theory. Or will Tolman assume that the goal loses its valence more or less if subordinate valences are distributed to the route?

Yet one could say that I used a similar concept to characterize the social influence, namely, the concept of the *"inducing field"* (9, p. 115). But here there is not the same difficulty, because the center of the inducing field itself (for instance, an adult) has, as such, no positive or negative valence, but only the induced valence (for instance, a given task or a forbidden path, for the child). We used the inducing field, recently, in connection with the theory of substitute and tools (3, pp. 46 f). It may be possible to interpret the path as a tool. In so doing, our theory, also, would have to speak of induced valences of paths. So, indeed, for us too there exist some difficulties which I think can be overcome only when one considers carefully whether and to what extent the goal, or only the tool, exists psychologically at a given moment. Above all, one has to determine the directions and paths not physically but psychobiologically in the meaning of hodological space. (See pp. 338 ff.)

I think that here, in handling the path problem, we touch upon one of the main differences between Tolman's theory and mine. Tolman speaks of the subordinate value of the path and of its various segments in every case in which the path and its segments exist ***physically***. But it seems to me that one has no right to do so.

A person may want to drive in his car from his home (H) to another house (B). Between H and B there may be two or three different roads. It may occur that he compares the roads and decides which road is more convenient. In such cases the paths between the person and the goal exist as such, psychologically, and the various paths may have different valences. A similar situation may exist for the rat if it has repeated the paths of a maze many times. In a maze also there exist real routes, that is, regions of the fields which allow locomotion in one dimension only. Locomotion in other directions is limited at most points by restraining forces. But the situation in free fields seems to be quite different.

The child or the animal may perceive the goal, may appreciate the direction of the goal and, more or less clearly, the distance to the goal. This, in itself, is enough to effect locomotion in the direction of the goal, above all, if it has a strong valence: A child six months old, for example, who cannot yet crawl and may not have insight into the use of tools or paths in reaching the goal, will, under given conditions, draw himself with his whole body directly toward the goal or make other *"diffuse-actions in the direction* of the goal." It seems to me that the experiments concerning actions, emotions, and expressions and our knowledge about the development of the individual show that these "diffuse actions toward or away" are the basis for the directed behavior of man and animal. Only comparatively late in the development of the child and under special conditions do "paths" exist psychologically as tools which have positive or negative valence.

At least in the case of free fields the concept of distribution of demand-value to the different segments of the path, as Tolman applies it, is inadequate and leads, as we have seen, to incorrect results.

VIII. The Cognitive Structure of the Field and the Direction of the Forces

To explain why, for instance, it is more difficult to find the roundabout route with such a barrier as that shown in Figure 7 rather than with the barrier as in Figure 8, it is necessary to discuss the relation between the directions of the field-force and the direction of the first part of the necessary roundabout route. Other things being equal, the solution is more difficult the more these two directions are opposed to each other. To define the psychological directions and their ratio one can use the physical directions only

in special cases and only to a certain degree, only when the physical directions are adequate to the psychological directions and to the degrees that they are adequate. This is one of the applications of the general rule that one has to define the psychological environment not physically but psychobiologically.

If one defines the direction psychobiologically, one understands, for instance, why the difficulty of the roundabout route for a child behind a U-formed barrier no longer exists for an older child if he has enough of a survey of the situation: The first part of the roundabout route for the older child has then no longer the character of a route *away* from the goal (as for the younger child), but *to* the goal (9, pp. 105 ff.).

Tolman thinks that I am inconsistent here in my use of the concept of vectors. First, I would like to say that I agree with Mr. Tolman that there are cognitive processes which have changed the structure of the field for the older child as compared with the younger child. Indeed, I have emphasized the fact that insight can be the reason for this changing of the structure and we believe that we can define rather exactly what kind of change occurs in this case at the moment of insight.

If one were to repeat in greater detail the explanation given in my earlier paper for the difference of the younger and older child or, better, for the changing of the structure at the moment of insight, one would have to say: Before the insight, the field which exists psychologically for the child is separated by the barrier into two unconnected regions (*A* and *B*, Figure 9) so far as locomotion is concerned. The force has the direction of the vector (*v*). Any movements in the opposite direction (*m*) have in this field the meaning for the child of the direction away from the goal.

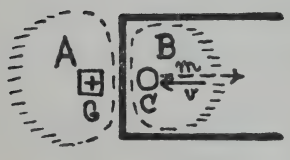

FIGURE 9

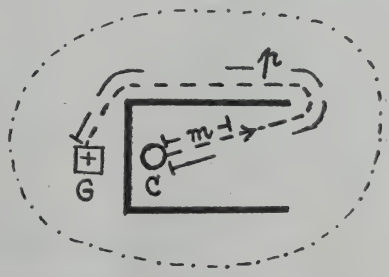

FIGURE 10

After the insight, the region of the goal and the region of the child are, concerning locomotion, *one* connected field. (Figure 10.) The path *m* has now psychologically the meaning of the first part of the whole path *p*.

The changing of the cognitive structure of the field is therefore, in accordance with Köhler's theory about any case of insight, a changing of the dynamical wholes in the field: The regions of the goal and of the child, which are first separate regions, are now parts of one unseparated region and the path *m*, which first was a path away from the goal, is now a part of the path *p*. From these facts one can understand why every factor which imposes greater limitations on the field psychologically existing for the child (for instance, strong emotions, or too high a valence, too inflexible character of the person, or special fixed habits) makes this changing of the structure of the field more difficult, and that every factor which enlarges this field (which gives the child a greater survey) makes the solution easier.

The direction of the field, as we have seen, is to be defined psychologically, and I think that Tolman will agree with me in principle at this point. After the changing of the structure of the field, the *direction m* is psychologically no longer a direction away from the goal but *toward* it. Therefore, the direction of the psychological *force to* the goal is then to be characterized as the direction of *m*, and the child is moving in the direction of this force to the goal in accordance with our basic theory about the connection between locomotion and forces. [Other examples of differences between the physical and the psychobiological directions have been given (9, p. 105).]

In reality, the situation is more complicated. For the functional connection between the goal and the child is not only a connection through possible bodily locomotions, but also a visual connection. So we have to deal here with the ratio of two fields in which the direction and distances are to be defined through different psychological functions. I hope I will have the opportunity to treat this material more in detail, and will limit our discussion here to the general idea of direction.

IX. Hodological Space, Stable and Unstable Equilibriums—The Idea of Resultant of Forces

The concept of direction in psychological space is undoubtedly one of the most difficult. The idea of direction has meaning only if

one can distinguish between different directions. Tolman says it may be difficult to define a perpendicular direction; and I too have emphasized the great difficulty in defining direction in the psychological fields. A person, for instance, may create some activity in the direction of some social position. Suppose we have the statement: "The person has changed the direction of his activity"; how are we to define these directions and how can we measure the angle between them?

Psychology seems to be in a very uncomfortable position in regard to these problems. On the one hand, the difficulty of defining directions in the psychological field seems to be so great that one wishes to avoid these concepts. On the other hand, I think it can be shown that no dynamic theory can avoid the use of these concepts. Tolman also uses direction as one of his basic concepts, but without being able to compare angles in the psychological field.

It would be possible to define angles if one could measure distances in the psychological field, but here we have similar difficulties: It seems to be possible only to use the relation greater and smaller and only if one compares distances in the same path and to the same goal. To some degree it is possible to give a general mathematical solution using a kind of space which is defined through the possible paths between points, and which one may call *"hodological space."* This space is more general than the space of Euclid and Riemann, but not as general as topological space, in which it is not possible to define distances or directions. We have not space to discuss the matter in greater detail and must limit ourselves to the consideration of some special cases which are related to Tolman's objections.

Criticizing my explanation of a conflict situation in which a child stands *between two negative valences* and shows a tendency to go sidewise out of the field, he says: "It is only by drawing the two opposing vectors V_t and V_p at a slight angle (which does not accord at all with this previous practice and implications) that he is able to indicate any such sidewise components. In all the previous examples vectors have been essentially perpendicular. It was because they *were* perpendicular that the organism was not able to pull away from them and go by indirect routes. But now it is admitted that vectors have some sidewise components. The doctrine we again must conclude is confused and misleading" (13, p. 12).

If we eliminate the confusing use of the term "perpendicular," which we have discussed before, the difficulties which Mr. Tolman

FIGURE 11
(Situation A)

FIGURE 12
(Situation B)

finds are quite easily solved. We may start with an example in which the psychological structure of the field is similar enough to the structure of the quasi-physical field so that we can use the direction of this field to define the direction of the psychological field. Let us compare two conflict situations, *A* and *B* (Figures 11 and 12). In *A*, the child (*C*) may stay between two positive valences (G_1 and G_2), in *B* between two negative valences. The distances and strengths of valences may be the same. If we draw the forces existing in these situations, both seem at first to be exactly the same for the child. But the behavior will be different, doubtless. In *B* one will find, for instance, a strong tendency which does not exist in *A* to go sidewise out of the field. The reason for this difference is clear if one considers the whole force-field according to our definition of valence.

The *force-field,* as we have indicated above, shows the distribution of forces for any given point of the region which would be existing at that point if the person were there. In the conflict situation *A*, we have to deal with the overlapping of two central fields. In the conflict situation *B* the forces are directed away from the location of the two negative valences.

The result of this difference one sees best if one considers the forces at the points of the imaginal line *S* (Figures 13 and 14). In the situation *A*, these vectors, v_1 (which corresponds to G_1) and v_2 (which corresponds to G_2), have a resultant directed to the connecting line between the two valences. This means, in the behavior of the child, that if he makes some sidewise movements there are forces which put him back in the field. On the contrary, the direction of

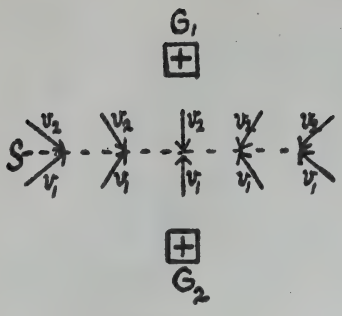

FIGURE 13
(Situation A)

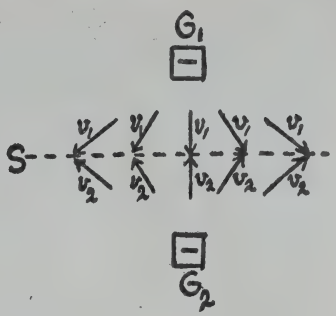

FIGURE 14
(Situation B)

the resultant vector, v_1 and v_2, in Figure 14 is away from the connecting line between the two valences. Therefore, if the child in this situation makes some sidewise movements, the resulting force pulls the child out of the field. It is true, as Tolman emphasized, that we cannot speak of this sidewise resultant if the child stands between the valences G_1 and G_2 so that v_1 and v_2 are just opposite in direction. For in both cases there exists an equilibrium. But in case *A* we have to deal with a *stable equilibrium* in so far as sidewise components are concerned. But in case *B* an *unstable equilibrium* exists, so that the incipient sidewise movements, which in every case exist (by chance or as restless movements), will destroy the equilibrium and result in going out of the field.[10] [In the figure (9, p. 111) which Tolman criticized (13, p. 11), I expressed this fact, referring to a position of the child beside the connecting line between the two valences.]

It is true that we use the idea of *resultant of forces,* and I think that only if one uses the idea of the resultant the concept of forces and the concept of directions can be useful in psychology. Above all, it is impossible to use the important concept of *equilibrium, without presupposing the idea of a resultant of forces.* Hence the equilibrium is only a particular case of such a resultant. It may be that in psychology we shall yet have a theorem like that of the parallelogram of forces in physics. Such a theorem includes the possibility of

[10] In reality one undoubtedly has to consider not only equilibrium concerning sidewise motions, but also concerning the directions G_1 and G_2, which is quite different in the different individual cases.

measuring angles, which at least does not now exist. Here again we have to deal with the general difficulty in defining directions.

Nevertheless, in some social situations the "opposite" and the "sidewise" directions can be defined. In the case of punishment, for instance, to which Tolman referred, one can characterize the sidewise locomotion as motion that leads spontaneously away from the disagreeable task and the punishment. In such a case, we can distinguish, therefore, three directions, or better, sectors of direction: the direction *to* G, the *opposite* direction, and the *sidewise* direction. Also Tolman, in employing the concept of direction, used the concept of sidewise locomotion, and, as far as I can see, his criteria are the same.

X. Summary

In summary, I want to emphasize first some important similarities between Tolman's theory and mine. The concept of vectors does not mix together in an unclear way the valences and that structure of the field which Tolman coordinates with "cognitive processes." Tolman's theory and mine are at this point very similar.

A main conceptual difference between our theories is, I think, that Tolman, starting from situations with comparatively few degrees of freedom, is basing the general theory of psychological movements on the idea of choice between *paths* with different *subordinate* demand-values. I, on the other hand, starting from free-field situations, use as a basis the concept of force or force-field, characterized through vectors.[11] Tolman's theory is, despite certain difficulties, I think, applicable in particular cases. Its general use, however, results in serious errors. On the other hand, in cases where his theory is applicable, I am convinced that one can derive it from our general thesis.

I think that even in such cases in which the concept of "path" in its real psychological sense is applicable, Tolman's basic statement that the direct path has more subordinate value than the indirect one is not true in every case, at least if we apply this concept as Tolman is doing. The statement is probably only valid then if there exists a tendency to reach the goal as quickly as possible (this tendency, undoubtedly, exists in any experiments with mazes to which Tolman

[11]The driving force has, in this concept, formally the position of force acting from a distance.

refers). The theory of forces, on the other hand, also permits the treatment of cases in which this tendency does not exist.

The general difference between our theories seems to be not so much a difference in "principles" as in the purity of application. We both define the psychological environment not physically but psychobiologically, and we both differ in this point from the older theory of tropism. Nevertheless, when applying, for instance, such concepts as that of path, Tolman seems to fall back, sometimes, into unbiological determinations.

He does not seem to distinguish clearly enough the question as to the effect on behavior of a field of given structure, and the more historical question as to why this field now has this structure.

Above all, the task of analysis, for me, seems not to be to find "mechanisms" "behind" the psychobiological field, but to determine the dynamical facts of the psychobiological field in such a way that one can derive logically all of the possibilities of actual behavior. I believe that for this purpose the use of qualitative and quantitative mathematical concepts is unavoidable.

Lastly, let me emphasize how important for the general theory is the cooperation between human and animal experimentation, and how much the work of Tolman contributes to this task.

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LES VECTEURS, LES PROCESSUS DE LA CONNAISCENCE ET LA CRITIQUE DE M. TOLMAN

(Résumé)

Les theories de M. T. et les miennes ont beaucoup de points de rapport; surtout l'idée *"demande-value"* de M. T. ne fait qu'une avec mon idée *"Aufforderungscharakter"* (valeur de la demande). La "Gestalttheorie" n'est pas ennemi de l'analyse, et ma *Feldtheorie* (théorie du champ) cherche à aller plus loin dans l'analyse mathematique que M. T.; mais elle ne cherche pas à passer à quelques entités, qui n'ont pas de position dans le champ lui-même. M. T. ne peut pas éviter les idées *"Kraft"* (force) et "Vector" (vecteur), et surtout pas l'idée de la *direction.* Il faudrait définir psycho-biologiquement les directions (avec l'aide de *"l'espace hodologique"*) et non pas physiquement, ce que les collaborateurs de M. T. n'observent pas toujours suffisamment. Pour éclaircir une quantité de cas la structure du *"Vector-Feldes"* (champ des vecteurs) est soumise, dans les phases diverses de l'action chez l'enfant et chez l'animal, à une analyse comparative. Nous discutons la relation entre "Zwangskraft" (force contraignante) et "treibende Kraft" (force poussante), leur effet sur "Unruhehandlungen" (les actions troublées), et le changement de la structure du "Kraftfeldes" (champ des forces) au moment de l'acte de l'intelligence.

LEWIN

VEKTOREN, ERKENNTNISPROZESSE UND MR. TOLMAN'S KRITIK

(Referat)

Tolmans und meine Theorien haben wesentliche Gemeinsamkeiten; insbesondere ist T's Begriff *"demand-value"* identisch mit meinem Begriff *"Aufforderungscharakter."* Die Gestalttheorie ist nicht analysefeindlich und meine *Feldtheorie* versucht in der mathematischen Analyse weiter zu gehen als T.; aber sie versucht nicht, auf irgendwelche Wesenheiten zurückzugreifen, die keine Stelle im Felde selbst haben. Die Begriffe *Kraft* und Vektor, insbesondere der Begriff der *Richtung* konnen auch von Tolman nicht vermieden werden. Die Richtungen sind (mit Hilfe des *"hodologischen Raumes"*) konsequent *psycho-biologisch* und nicht physikalisch zu definieren, was von T's Mitarbeitern nicht immer hinreichend beachtet wird. Für eine Reihe von Einzelfallen wird die Struktur des Vektor-Feldes in den verschiedenen Phasen des Geschehens bei Kind und Tier einer vergleichenden Analyse unterzogen. Der Zusammenhang von "Zwangskraft" und "treibender Kraft," ihre Wirkung für die Unruhehandlungen, ferner die Umstrukturierung des Kraftfeldes beim Akt der Einsicht werden erörtert.

LEWIN

STUDIES IN LINGUISTIC BEHAVIOR ORGANIZATION: I. CHARACTERISTICS OF UNSTABLE VERBAL REACTIONS*[1]

From the Psychological Laboratories of the University of Illinois

ERWIN A. ESPER

INTRODUCTION

The general nature and the history of the experimental problems under investigation in these studies, as well as the technique devised for the purpose, have been described in a previous publication (2).

A striking feature of language is its compromise between stability and instability, and between specificity and generalization. Did not the stimulus-response relationships remain fairly stable and specific on the part of most members of a speech community during a given historical period, social interaction as we know it would be impossible. And yet the history of languages is a record of shiftings in stimulus-response relationships, and even within any given brief period it is obvious that the specificity is a relative matter; that we cannot analyze the environment into unit patterns and expect to find for each of these patterns a verbal response which is never elicited by other patterns. This means that there must be much variation of response even if the environment were to remain constant. And this variation of response seems to lead to a gradual assimilation of the categories of language to what might be called the 'objective categories' of the environment; that is, the overlap-

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[1]This experiment was carried out in the Psychological Laboratory of the University of Illinois during the academic year 1925-26.

Since the completion of the manuscript of this article, I have found in a volume edited by W. L. Valentine (7) an article by D. Wolfle (8), which is essentially a repetition of the experiments which I published in the monograph (2), with some additional features of method and interpretation whose resemblance to some of those of the present article may seem striking in the absence of any explanation by Mr. Wolfle. As a student in my laboratory, and in subsequent correspondence, Mr. Wolfle was made acquainted with much of the material here presented.

pings of patterns, the recurrences of common features of color, shape, sound, etc., come to be reflected in the interrelationships of the verbal units.

A serious fallacy which seems implicit in much pseudo-psychological theorizing in the field of linguistics consists in the assumption that linguistic material can be adequately studied without reference to the stimulus settings in which it occurs. This assumption proceeds from the notion of language as being the 'expression of ideas.' Actually, I do not see how we can escape the view that language, in its fundamental, living form, consists of a system of verbal responses to (chiefly external) stimulus patterns, closely interrelated with the patterns of manipulative behavior.[2] The fundamental unit to be investigated then is not a purely grammatical one, but a *stimulus-response unit.* It is not enough to say, e.g., that a certain word 'tends to influence the form' of another; this is a shorthand statement which conceals the real problem. Words and word-mixtures occur as responses to situations, and changes in the form or meaning of words are only one aspect of a changed relationship between certain patterns of stimulation and the response patterns of the individuals of a speech community. It is from a study of the conditions determining these changes in stimulus-response relationships that we may hope to arrive at the principles of linguistic change and of linguistic organization.

In the present experiment, the conditions provided were as follows: (*a*) verbal responses were attached to a number of stimulus-objects which resembled one another in shape or size, but which were so selected that there was a certain asymmetry in the objective classes to which they belonged; and (*b*) additional objects were subsequently introduced which systematically resembled the original objects but for which no specific names had been learned. In both cases the purpose was to provide conditions favoring varying degrees of instability in the stimulus-response relationships. Two main problems were under investigation: (*a*) what are the conditions determining relative stability and instability in verbal responses, and what characteristics differentiate stable from unstable stimulus-response units; and (*b*) in what manner and as a result of what changes

[2]This matter has been ably presented by Dr. Malinowski in his essay on "The Problem of Meaning in Primitive Languages," printed as a supplement to Ogden and Richards' *The meaning of meaning* (6).

does an unstable linguistic system tend toward an equilibrium among its component stimulus-response units.

Method

The general method consisted of (1) teaching the S's to respond to 'nonsense' figures with 'nonsense' names,[3] continuing the repetitions to the point of 'complete' learning; and (2) thereafter requiring the S's to continue to respond to the figures under various conditions.

At the beginning of the experiment the following instructions were read to each S:

> "I am going to teach you the names of a number of figures. As each figure appears, I shall pronounce its name. You will repeat the name after me, aloud. After each 4 repetitions of this procedure, I shall say 'call them out.' You will then attempt to call out the names of the figures as they appear, in a loud, distinct voice. If you are not sure of the names, guess. Be careful to pronounce the names exactly as I do. Do not be discouraged if you unable to learn them at once."

Before proceeding according to these instructions, E pronounced each of the names a number of times, permitting S to watch his lips, until S could repeat the names accurately (or as accurately as dialect differences permitted).

The apparatus used for presenting the figures is the 'bradyscope,' which has been described elsewhere (3). The figures were presented at the rate of 1 every 3 sec.; the change from one figure to the next occupied 1 sec. The bradyscope was placed at one end of a 10-x-7-ft. darkroom, the interior of which was painted a uniform flat gray. The apparatus was illuminated by a 100-watt 'daylight' lamp in a large reflector suspended above and behind the S's head. S sat at a distance of 1.5 m. from the apparatus. To his

[3]The word *nonsense* is used for want of a better. By a nonsense figure or object I mean one that has been artificially constructed in such a manner that it will have a minimum of resemblance to anything encountered in a subject's previous experience; by a nonsense word I mean a sound sequence which differs as much as possible from any word of actual language. Obviously, when subjects have been induced over a period of many months to attach certain names to certain figures, neither the names nor the figures can be said to be 'nonsense' in the sense that the term is used when we speak of Ebbinghaus' syllables. Consequently the criticisms which have been directed at the use of 'nonsense syllables' by various writers have no applicability to the materials used in these experiments.

right was E's table on which were the apparatus controls, screened from S by a hood. E sat at the end of this table, to S's right and rear.

As is shown in Figure 1, the S's were taught names for 12 figures, figures A1, B2, C3, and D4 being omitted during the first period of the experiment. I shall designate as a *series* the presentation of the 12 figures in random order, each figure occurring once in a series. I shall designate as an *L-series* (learning series) a series in which E calls out the names of the figures and S repeats them. I shall designate as an *R-series* (recognition series) a series in which S calls out the names without assistance from E.

Four series, representing four different haphazard orders of the figures, were in the bradyscope at each sitting. For each sitting four new haphazard orders were provided. On the first day, S was given Series 1, 2, 3, 4 as L-series; then Series 1 as an R-series; then Series 2, 3, 4, 1 as L-series; then Series 2 as an R-series. This procedure was repeated on succeeding days until he was able to name correctly six of the figures. Thereafter S was given alternately two L-series and one R-series until he could name eight of the figures. From this point he was given alternately one L- and one R-series until he could name all of the figures correctly in four successive R-series. To reach this point required for the various S's from four to ten sittings. The sittings occurred on five days of each week. Each sitting was terminated when a total of eight L-series had been given.

The *subsequent* history of the experiment is divided into two periods.

Period I. This was a period of about four months, from the first week of November to March 1. Up to January 20, the S's appeared on five days of the week; thereafter on three days each week. During this period the S's continued to name the 12 figures in four R-series at each sitting, being given no further L-series unless they made *uncorrected* errors. Slight indispositions of the various S's, vacation periods, etc., of course interfered with the routine of sittings; detailed information concerning the actual distribution of sittings in Period I for each S may be obtained from Figure 3.

Period II. This was a period of about three months, from March 8 to May 24, during which sittings occurred on three days of each week. No further L-series were given. During this period 16 figures were being presented, the 4 figures previously omitted having

been introduced. These 4 figures will hereafter be referred to as the *N-figures* (new figures), and the other 12 figures will be called the *O-figures* (old figures).

Period II is divided into two stages:

Period II-a. March 8 to April 14. The N-figures were introduced. Further, instead of the figures being presented in four series as previously, they were presented in one continuous series in which each of the figures (O and N) occurred four times in haphazard order. Thus the S's no longer had the assurance that when a given figure had once appeared in a given series it could not again occur in that series. The following instructions were read to S:

> "From now on the figures instead of being arranged in four separate series will be presented in one continuous series in which each kind of figure will occur a number of times. Be sure to name each and every figure promptly as soon as it appears, even though some of the figures may appear to be not quite the same as previously. Do not let a figure pass without naming it."

Period II-b. April 16 to May 24. A second variation of conditions was introduced. This consisted in presenting the figures in pairs, one on the left side of the bradyscope, the other on the right. The S's were required to name both figures, the one on the left first. Each figure was paired with each other figure, and each pair was presented with sometimes the one and sometimes the other figure in the left-hand position. In the paired series arranged in the bradyscope for each sitting each figure (O and N) occurred four times; the entire series was given twice, so that at each sitting the S's responded to each figure eight times (as compared with four times in the preceding periods). Since the speed of the bradyscope remained the same as before, the paired manner of presentation also had the effect of decreasing the time available for the response to each figure.

Throughout all periods of the experiment, E recorded all responses of the S's in phonetic symbols.

Measurements of Reaction-Time and Associations. On three occasions during Period I (Dec. 18, Jan. 21, March 3) and on three occasions during Period II (March 29, May 7 and 26), the reaction-times of the S's in responding to each of the figures were measured, two measurements being made for each figure on each oc-

casion. Thus for each S (with exceptions noted below) there were obtained six measurements for each figure in each of the two periods of the experiment. During Period II the reaction-time for the N-figures were also measured. In addition, on each occasion the reaction-times of verbal responses to 12 familar objects were measured, in order that these times might be compared with those for the experimentally learned responses. The following instructions were read to S:

> "I shall show you a series of figures. Some of these will be the ones whose names you have learned in this experiment; others will represent the following objects: table, house, pipe, duck, glove, hat, chair, shoe, umbrella, tree, bell, broom. As soon as you catch sight of each figure, call out its name as quickly as possible. I am going to measure the time it takes you to call out each name."

The nonsense figures and those representing the familiar objects (the latter cut from white Whatman paper and mounted on gray cards in the same manner as the nonsense figures) were then presented in haphazard order, each figure occurring twice.

Likewise on three occasions during Period I (Dec. 21, Jan. 22, March 5), and on three occasions during Period II (March 31, May 7 and 26), association measurements were made. The stimulus words were the names of the nonsense figures and in addition 12 English words (*father, brother, young, black, one, nine, this, he, eating, reading, now, seldom*). These were given in random orders, each word occurring twice at each sitting, so that six measurements were obtained for each word in each period. The inclusion of the English words was for the purpose of comparing the experimentally established associations with those obtaining in actual language. The following instructions were read to S:

> "Please keep your eyes closed throughout this experiment. I shall pronounce a series of words. Some of these will be ordinary English words; others will be the names of the figures which you have learned. As soon as you hear each word, call out as quickly as possible the first *other* word that pops into your head as a result of hearing the word that I pronounced. The first five words will be for practice."

The practice words *house, jumping, fast, red, who* were given, followed by the test series.

For the reaction and association time measurements, voice-keys

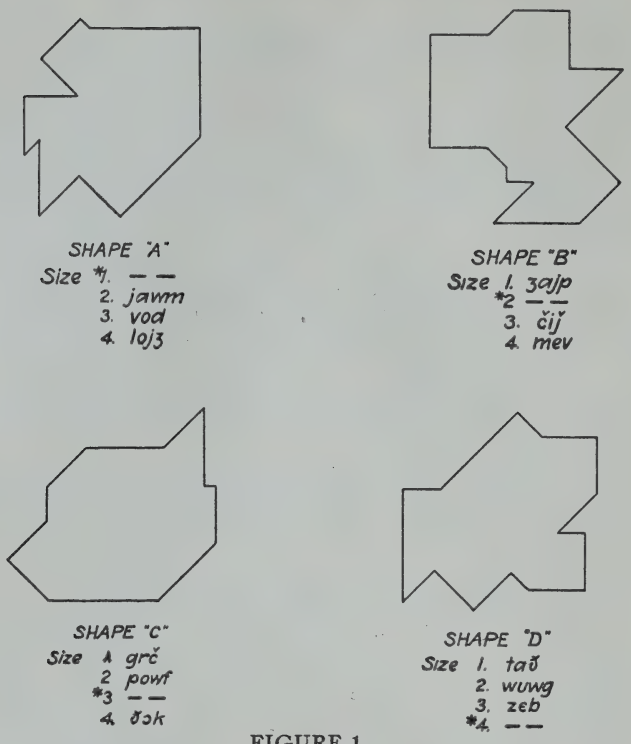

FIGURE 1

and a Dunlap chronoscope, the latter reading in 1/100-sec. units, were used. S and E sat in the darkroom; the chronoscope was located in another room, the readings being taken by an assistant. In the reaction-time measurements, the chronoscope was started by the falling of the bradyscope shutter; in the association measurements, by E pronouncing the stimulus word against the stimulus voice-key. In both cases, the chronoscope was stopped by S speaking his response against the reaction voice-key.[4]

The Nonsense Figures. The figures varied as to shape and size,

[4]A voice-key with the ordinary tripod base, standing on a table, is very likely to be tripped by floor vibrations. This difficulty was overcome by inverting the base, fitting the feet with levelling screws, and suspending the base from the ceiling of the outer room by piano wires which passed through the ceiling of the darkroom; the wires were provided with steel springs about 2 feet above the voice-key base.

there being four categories of each of these. The areas of the four sizes are geometrically related, as follows: size 1, 8 sq. cm.; size 2, 16 sq. cm.; size 3, 32 sq. cm.; size 4, 64 sq. cm. These sizes will be symbolized hereafter by the numbers indicated; the shapes will be symbolized by the capital letters *A, B, C, D.* Thus *C1* will refer to a figure of shape *C* and of area 8 sq. cm. (See Figure 1.) The designations of the N-figures will be starred, thus, **A1.*

The 4 shapes used in this experiment were selected from 80 shapes obtained from various combinations of the Chinese tangram figure. These 80 shapes were presented to 40 individuals who were instructed to record the objects of which each figure reminded them. In the case of each of the shapes selected, almost two-thirds of these individuals were unable to note any resemblance to an object; the responses of the others were highly individual and variable. These probably represent nearly the optimal conditions attainable with 'nonsense' figures.

The figures were cut from white Whatman paper, which has an excellent non-glossy surface, and were mounted on medium gray cardboard.

The Nonsense Names. The names assigned to the figures were selected from a list of all the possible monosyllabic combinations of the distinctive speech sounds of Middle-Western American English. From this list were chosen 200 syllables which had the least tendency to suggest actual words to myself and my assistant. These 200 syllables were then orally presented to 48 individuals with the request that they record any words of any language that the syllables suggested. On the basis of the results thus obtained, 12 syllables were chosen which (*a*) differed as much as possible in initial, medial, and final sounds from one another; and (*b*) had the least tendency to suggest actual words.

The names assigned to the different figures are indicated in Figure 1.[5] To avoid the necessity of printing phonetic symbols in the text of this article, the names will hereafter be referred to by giving, in single quotation marks, the shape-size designation of the figure to which the name was assigned *in the original learning,* as shown in Figure 1. Thus if we state that an S responded to C1 with 'C2,' this will mean that he called the shape-size combination *C1* by the name

[5]The values of the phonetic symbols are the same as those indicated in a table appended to my monograph (2).

originally learned for *C2*. This procedure is possible because in only a few isolated cases did the S's momentarily depart from the original phonetic forms of the words.

It will be seen in Figure I that the material selected provided no correlation between the phonetic forms of the names and the categories of shape and size in the figures. Every name is as different from all the others as to initial, medial, and final sounds as it was possible to make it. The words therefore constitute a *totally suppletive system.*

The Subjects. The 12 subjects were undergraduate students in the University of Illinois, of ages between 20 and 24 years. S's 1, 4, 7, and 9 are women. A larger number of S's were started in the experiment, but only 12 completed the various stages of the experiment, which extended over some 8 months.

Results in Period I

Table 1 summarizes the results in Period I for 8 of the 12 S's. The results for S's 5, 6, 10, and 11 are not included in this table because of the nature of their associative responses. The instructions for the association tests required the S's to respond with the *first other word* that occurred to them, and the 12 nonsense words were presented in a series in which 12 English stimulus words also occurred. The S's were therefore free, so far as the experimenter's instructions went, to respond to the nonsense words with English words or words of any other language, if these should be the first to "pop into their heads." The purpose was to test the functional facility of the artificial linguistic material; to determine to what extent these nonsense words would elicit 'spontaneous' functional associations conforming to their own categories, when presented without restrictive instructions and in temporal proximity to English stimulus words and responses. It was also thought desirable to determine to what extent the nonsense words might have become associatively linked through possible sound similarity or other factors with English or other words. Under these conditions, seven of the S's were found to respond to the nonsense words exclusively with other nonsense words of the system; S 3 did so with the exception of his responding to *mev* ('B4') with *zev,* the name of a racehorse with whose contemporary career this S had become financially involved. S's 5 and 10 responded exclusively with words other than those of the artificial system; S's 6 and 11 did so in 64 and 29%

TABLE 1

Summary of Results for Eight Subjects in Period I

The columns, numbered from left to right at the top, contain the following data: (1) the figure designations; (2) the average number of L-series required for the first correct naming of the figure; (3) the average reaction-time in naming the figure; (4) the total number of errors made by all S's; (5) total number of failures to respond; (6) the most frequently occurring error, and (7) its frequency; (8) the most frequent associative response to the *name* of the figure, (9) the number of occurrences, and (10) the average reaction-time of these associations; (11) the number of other associative responses, and (12) their average reaction-time; (13) the average reaction-time of *all* the associative responses to the various figure names; (14) the number of times that the name of the figure indicated in column 1 occurred as an associative response to all of the other figure names, and (15) the average reaction-times of such occurrences.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Fig.	L's	RT	Er.	F.	M. f. er.	N		M. f. assoc. N	T	Others N	T	All	As N	Assoc. T
A2	26	111	32	6	'A3'	28	'A3'	42	118	6	127	119	26	118
A3	31	103	23	3	'A2'	16	'A2'	20	116	28	123	120	77	122
A4	17	87	3	1			'A3'	22	123	26	145	135	13	108
B1	19	99	12	2	'B3'	9	'B3'	35	93	13	120	100	20	124
B3	11	99	16	1	'B1'	13	'B4'	22	93	26	109	102	90	95
B4	18	85	4	0			'B3	16	121	26	135	130	41	103
C1	19	98	9	1	'C2'	7	'C2'	42	91	5	121	94	48	135
C2	21	96	13	1	'C1'	9	'C1'	18	111	29	127	121	90	103
C4	16	86	7	1	'D1'	6	'C2'	21	101	27	132	119	24	129
D1	37	92	12	1	'D2'	8	'D2'	41	90	7	143	98	27	133
D2	37	103	21	6	'D3'	10	'D1'	16	118	16	117	117	93	99
							'D3'	16	116					
D3	18	110	25	3	'D2'	18	'D2'	26	95	22	123	108	27	129

of the cases, respectively. Since the relation of the associative responses within the system is one of our primary interests, the results of the latter four S's have been omitted in the summary of Table 1.

The table reveals the following relationships:

1. *Relative Stability and Patterning of the Categories.* The data of columns 3 to 15 represent the S's performance during Period I; that is, *subsequent* to their having satisfied the criterion of four errorless R-series. It will be seen that the number of errors is extremely small, and that they are due primarily to the experimentally provided factors of instability in the system. Categories A and D show much more instability than do categories B and C. In comparison with the corresponding figures of categories B and C, figures A2, A3 and D2, D3 produce longer reaction-times and a larger number of errors and failures, the errors consisting chiefly in a confusion between A2, A3 and D2, D3. Figures A2, A3, and D2 also require a larger number of L-series than do any of the other figures. Figures A4 and D1, however, behave in a manner similar to that of the corresponding figures of the other categories, showing relative stability. These facts may be referred to the following conditions:

a. The relation of the figures to their backgrounds. The figures of size 4 occupied most of the exposure area, with a relatively narrow margin of gray cardboard around their edges; the other figures, in comparison, tended to be 'white figures in the center of a gray card.' In consequence, the responses to the figures of size 4 show marked differences from the responses to the other figures. They are learned more quickly; their reaction-time is distinctly shorter; they induce fewer errors and practically no failures to respond; there is no one most frequent error except in the case of C4, where, however, the number of occurrences is small and is due to the sound similarity between 'C4' and 'D1'; the associative responses to the names show considerable scatter and long reaction-times. Thus the responses to these figures show great stability and facility.

The figures of size 1 had, in comparison with the other sizes, somewhat the appearance of 'miniature' figures in the center of the gray cards. They were not thereby marked off from the other figures as distinctly as were the figures of size 4; they did not require fewer L-series than did sizes 2 and 3, nor were their reaction-times shorter. They did, however, induce a somewhat smaller number of errors than did sizes 2 and 3. The greater distinctness of the

size-4 figures is also shown by the fact that very few of the errors in response to the other sizes consisted in calling them by the size-4 name, whereas 'B1' and 'C1' were in a number of cases elicited by B3 and C2, respectively.

The four sizes in which the figures occurred were in geometrical progression (8, 16, 32, 64 sq. cm.). The differences in area were therefore such that no difficulties of discrimination would be expected. But in relation to the background cards of uniform dimensions, sizes 2 and 3 would be psychologically much more similar to one another than would any pair of which size 1 or size 4 is one member. This fact would account in some measure for the longer reaction-times and greater number of errors found for A2, A3 and D2, D3.

b. The behavior pattern induced by the system. The conditions discussed under (*a*) do not appear to explain all features of the instability in categories A and D. In category A, size 2 has the longest reaction-time, and is called by the name of A3 more often than A3 is called 'A2.' But in category D, the same relations hold, *mutatis mutandis,* for *D3* as compared with *D2*. Also, the associations 'A2'→'A3' and 'D3'→'D2' are much stronger than the reverse associations.

These and other facts seem to be explained by the general patterning of the system as revealed in the subjects' responses. According to this pattern, the system consists of four shapes each occurring in three sizes: small, medium, and large. Except for certain differences in the A and D categories, due to the special conditions there, the smallest figures of each shape category (A2, B1, C1, D1), the medium-sized figures (A3, B3, C2, D2), and the largest figures (A4, B4, C4, D3) in each case constitute a group whose members tend to behave similarly.

1) The similarities in the responses to the *large* figures have already been discussed. D3 conforms to this pattern in the relatively small number of L-series required, but the reaction-time and number of errors is considerably greater than for the size-4 figures. It is not unlikely that the analogy of the other shape categories has an effect here; 'D3' is the name of the largest D figure, but when D3 appears, it does not 'seem large enough' and there is a tendency therefore to call it 'D2.' In category A as well as in D, sizes 2 and 3 are both present, and the name of size 3 is chiefly associated with that of size 2, but in the case of 'D3' the association is

stronger and has a distinctly shorter reaction-time; this may be due to the frequent tendency to respond to D3 with 'D2'; there were 12 cases during Period I where the overt response to D3 was 'D2,' immediately corrected (overtly) to 'D3.' The *implicit* cases of this sort were probably quite numerous.

2) The behavior pattern with respect to the *small* figures is distinguished chiefly by the associative responses to their names. The most frequent response to 'A2,' 'B1,' 'C1,' 'D1' is in each case the name of the next larger figure of the same shape; the frequencies are very much larger and the scatter decidedly less than for the responses to the other names. The average associative reaction-times are the shortest that occur, except in the case of 'A2.'[6] The small figures are learned less quickly than are the large ones, but their difference in this respect as compared with the medium-sized figures depends upon the relative conditions of equilibrium in the various shape categories. The reaction-time in naming the small figures is about the same as that for naming the medium-sized figures, except in the case of A2. Again with the exception of A2, the number of errors tends to be less than for the medium figures, but greater than for the large figures. A2 therefore conforms to the behavior pattern of the small figures only in the high frequency of its name's most frequent associate. Here again, as in the case of D3, the general pattern of the system seems to have its effect; 'A2' is the name of the smallest A figure, but when A2 appears, it is likely to appear 'too large,' and tends to be called 'A3.' This error is, in fact, made more often than any other, and there are 23 cases in Period I where the S's first called the figure 'A3' and immediately corrected themselves; there were even cases where the response was, e.g., *jaw-vo-jawm*. This figure therefore has a tendency to release *both* responses, and the resulting interference would account for the long reaction-time in naming the figure and *perhaps also for the associative frequency*. I shall discuss the latter point in some detail in the final section of this paper.

3) The behavior pattern for the medium-sized figures is characterized by a somewhat greater tendency to errors, but chiefly by the fact that the names of these figures constitute by far the most

[6]The fact that the association 'A2'-'A3' has, in spite of its frequency, a long reaction-time is probably due to some difficulty in discriminating the word when it was quickly pronounced in the association tests; some S's occasionally understood it as *young*, a word which also occurred in these tests.

frequent associative responses to the names of the other figures (see column 14 of Table 1), with (except for 'A3') the shortest average association times. This frequency comes about largely from the fact that in each shape category the names of both the small and the large figures are associated predominantly with the names of the medium-sized figures. It is to be noted, however, that the association *small→medium* is much stronger than the association *large→medium*. As for the associative responses to the names of the medium-sized figures themselves, we find 'A3' and 'C2' associated chiefly with the names of the small figures, 'B3' with that of the large figure, and 'D2' equally with those of the small and the large figures. We find also that 'A3' is associated 9 times with 'A4,' 'B3' 12 times with 'B1,' and 'C2' 9 times with 'C4.' Thus the situation presented by 'D2' is more or less general for the names of these medium-sized figures; they face both ways, tending to be associated with the names of both the small and the large figures.

2. *Conditions Determining the Associations.* We can now state the principles governing the establishment of the verbal associations as follows:

a. The greater the possibility of confusing an object a *with a specific other object,* b, *and the less the possibility of confusing it with any other specific object,* c, d, *etc., the firmer and more universal does the verbal association between the names of objects* a *and* b *become.*

The *small* figures are subject to confusion almost exclusively with the medium-sized figures. In accordance with this fact, the verbal associations small→medium come close to 100% occurrence.

The *medium* figures are subject to confusion with both the small and the large figures. In accordance with this fact, associations in both directions tend to be established, neither association being very strong. This comes out most clearly in the D category, where *D4 is lacking and D1, D2, D3 form a psychologically more similar series. D2 is overtly called 'D1' about as often as it is called 'D3,' and the associations 'D2'-'D1' and 'D2'-'D3' are of equal frequency and reaction-time. In the other categories, B3 is closer to B4 than to B1, and C2 is closer to C1 than to C4; the associations are in accordance with these relationships. The A-category might seem analogous to the D-category, but A4, due to its figure-ground individuality, has less tendency to enter into confusion with A3, and the latter's name is associated chiefly with 'A2.'

The *large* figures are subject to confusion almost exclusively with the medium figures, but their figure-ground individuality prevents this confusion from being of frequent occurrence. Consequently, while the most frequent associations are those with the names of the medium figures, the frequency is not large and the association times are long. The exception is D3, which, lacking the individuality of the size-4 figures, shows a stronger tendency to confusion with the medium figure and a consequently stronger association.

Thus the material clearly illustrates the operation of the two conditions stated in the principle. The names of the small figures elicit the firmest associations because these figures are capable of confusion with *one* other figure; the medium-figure names elicit weaker associations because these figures tend to be confused with *two* other figures; the large-figure names elicit weaker associations because these figures, though their only real possibility of confusion is with *one* other figure, yet possess such individuality that this confusion is of infrequent occurrence.

b. The weaker the tendency to confuse an object with any specific other object in its own immediate category, the greater is the tendency for its name associatively to elicit names from other categories.

The small-figure names are associated 27 times with the names of figures of other shape categories; the medium-figure names, 68 times; the large-figure names, 92 times. Of these occurrences, 33, 57, and 37%, respectively, are associations with the names of figures of other shape categories but of the corresponding size category. Thus associative interconnections with other categories are attributable chiefly to those stimulus-response units which are least definitely confused and associated with a specific member of their own category.

c. When a given stimulus-response unit has no strong tendency to be confused with any specific other unit of its own category, verbal associations on a purely phonetic basis may be established, regardless of the categories of the stimulus-objects themselves.

The stimulus word 'A4' elicits the association 'C1' four times for S 4 and twice for S 9; 'B3' elicits 'C1' three times for S 4, four times for S 9, once for S 12. All three of these S's had trouble with the voicing of fricatives, so that the final consonants of 'A4' and 'B3' were much nearer to that of 'C1' than for the other S's. The same explanation probably applies to the association 'B4'→'C2,'

which occurs twice for S 12 and exclusively for S 4. These occurrences indicate the importance of dialectal phonetic variations in furnishing foci from which levelling may spread.

d. When a given object is subject to confusion with a number of other objects, its name tends to become an alternative response to these other objects and an associative response to their names, and consequently to become the most frequently elicited name in the category.

To be noted is the peculiarly prominent position assumed by stimulus-response units having the relative positions of the medium figures and figure names of this material. For example, when either D1 or D3 appears, if any response mechanism other than, or in addition to, the one specific to the figure is activated, it is most likely to be the one corresponding to D2. Thus the 'D2' response tends to be activated not only by its own stimulus-object, but also by two others. The same is true, in varying degrees, for the response mechanisms corresponding to A3, B3, and C2. Moreover, the *names* of, e.g., D1 and D3, in consequence of the above relations, both tend associatively to activate the verbal mechanism 'D2.' The consequence is that the stimulus-response units corresponding to the medium-sized figures become, so to speak, the centers of gravity of their respective categories; they are the response mechanisms most frequently activated, and they are not only specific responses to a given object but alternative responses to the other objects of their category. The importance of this is understood when considered in connection with the finding in an earlier study (2) that names of the various objects of a category tend to become phonetically assimilated to the name of *one particular* object of that category. Of course, this tendency for a response to become an alternative response to other objects of the same category is at the same time an example of a 'generalizing' process.

However obvious these relations may seem, they are not so *a priori*. One might well have expected that the prominence of the *large* figures because of their figure-ground character, the readiness with which they were learned, and the short reaction-times required to name them, would have made them the foci of their categories. The principle which actually emerges, however, is that that response mechanism is most likely to become the center of a developing semantic-morphologic system which is most readily elicited as a 'confusion response' by the largest number of other stimulus-

objects. The special prominence of these response mechanisms is also seen in their tendency to be elicited associatively by the names of figures which do not belong to the same shape category, or in some cases even to the same size category. This is especially noticeable in the case of S 3, who gives the associative response 'B3' to the names of figures A3, A4, C2, C4, D2, D3, as well as to those of B1 and B4.

Results in Period II

In Table 2 are summarized the responses to the N-figures in the two stages of Period II, and in Table 3 are summarized, for comparison, the reaction-times and errors for the O-figures in *all* stages of the experiment.

In Period II are introduced the four N-figures. How will these be named? For *A1 and *D4 this can be predicted with considerable certainty; A2 and D3, respectively, alone stand adjacent to these figures in size. Moreover, the absence of the smallest figure in the A-category and of the largest in the D-category had had much greater effect on these categories in Period I than had the omission of *intermediate* figures in the B- and C-categories, so that already in Period I the name of A3 had shown a tendency to become attached to A2, and that of D2 to D3. And, in fact, Table 2 shows that 'A2' and 'D3' respectively were the almost invariable responses to *A1 and *D4, and that these responses had short reaction-times. These names thus underwent a semantic change, for which, however, the way had been prepared analogically in Period I by the presence of the size-1 and size-4 figures in the B- and C-categories. But as a result of this semantic change, a compensatory shift tends to occur; the tendency to call A2 and D3 by the names of A3 and D2, respectively, is greatly increased, so that by the end of the experiment this substitution is complete (invariable) for two of the S's (Nos. 4 and 7) for both figures; for S 2 it is complete for D3, and for S 11 it is complete for A2.

The responses to *B2 and *C3 could not be predicted with the same certainty. In the C-category, *C3 is intermediate in size between C2 and C4. However, C4 has the greater figure-ground individuality and the greater stability, as shown in Period I by the small number of errors and the short reaction-time. C2 is one of the more unstable medium-sized figures, with longer reaction-time and more errors. In accordance with these facts we find that *C3 is in about 90% of the presentations called 'C2.' However, it is also called

TABLE 2

Responses to the N-figures (All Subjects)

In columns 2 and 3 are given the average reaction-times in naming the figures in Periods II-*a* and II-*b*. Under *responses*, the first number following each response designation indicates the frequency in Period II-*a*, the second number the frequency in II-*b*. *F* = failures to respond. Frequencies are in terms of number of occurrences per 1000 presentations.

Fig.	Reac.-time II-*a*	Reac.-time II-*b*	Responses
*A1	90	81	'A2' (970,995); 'A3' (5,2); 'D1' (5,0); F (21,1).
*B2	110	90	'B3' (683,638); 'B1' (299,354); 'B4' (0,4); F (16,4).
*C3	106	90	'C2' (901,886); 'C4' (76,107); 'C1' (11,4); F (11,2).
*D4	103	82	'D3' (954,986); 'B4' (21,2); 'C4' (2,5); 'A4' (5,2); F (16,5).

'C4' on occasion, and S 10 gives it this name in 76% of the presentations and does so exclusively in the last 32 presentations.

In the B-category, *B2 is intermediate in size between B1 and B3. The latter is the more unstable, but B1 has not the same degree of individuality and stability as have the size-4 figures. Hence we are not surprised to find that while *B2 is called 'B3' in about 65% of the presentations, it is also called 'B1' in 30% of the presentations of II-*a* and in 35% of those of II-*b*. S's 3, 6, and 9 give *B2 the name B1 in about 90% of the presentations, S5 does so in 46%, while the others call it 'B3' in about 90%. In accordance with the situation thus outlined, we find that in II-*b* the average reaction-times for the responses to *B2 and *C3 are about 1/10 sec. longer than those for *A1 and *D4, and that, whereas the responses to *A1 and *D4 become more uniform in II-*b* as compared with II-*a*, the responses to *B2 and *C3 become more variable.

The effects on the O-figure responses of the introduction of the N-figures in II-*a*, and of the paired method of presentation in II-*b* may be seen in Table 3. The stimulus-response units whose stability is most affected by the introduction of the N-figures are indi-

TABLE 3

COMPARISONS OF THE RESPONSES TO THE O-FIGURES IN THE SUCCESSIVE STAGES OF THE EXPERIMENT (ALL SUBJECTS)

The frequencies of the errors for the three periods have been reduced to a common basis of *number per 1000 presentations.*

Fig.	Reaction-time			Errors and failures			Most frequent errors			
	I	II-*a*	II-*b*	I	II-*a*	II-*b*		I	II-*a*	II-*b*
A2	108	109	97	15	85	173	'A3'	11	83	166
A3	107	108	94	11	31	27	'A2'	5	18	11
							'A4'	2	6	9
A4	92	91	85	3	8	9	'A3'	1	6	5
B1	97	87	83	6	6	22	'B3'	4	4	13
B3	105	109	89	9	18	47	'B1'	2	4	12
							'B4'	4	10	31
B4	91	82	85	2	12	20	'B3'	1	8	13
C1	101	93	83	6	22	19	'C2'	6	20	12
C2	101	116	82	7	35	50	'C1'	5	32	45
							'C4'	1	0	1
C4	90	93	79	3	8	15	'C2'	1	2	9
D1	94	94	82	6	24	26	'D2'	3	24	20
D2	107	99	90	20	18	54	'D1'	6	12	30
							'D3'	6	4	11
D3	109	107	97	15	124	205	'D2'	7	120	194

cated in II-*a* not only by the increase in errors but also by the long reaction-times (figures A2, A3, B3, C2, D2, D3). The specific substitutions which chiefly tend to occur in the responses to these figures correspond to the points in each category-pattern occupied by the N-figures. A2 is intermediate between *A1 and A3; it must be called either 'A2' or 'A3,' since *A1 is regularly called 'A2.' The same is true, *mutatis mutandis,* for the responses to D3. Figure B3 is intermediate between *B2 and B4; i.e., between a figure called in most cases 'B3' and one called 'B4'; hence when it does not elicit 'B3,' it is more likely to elicit 'B4' than 'B1.' A similar explanation would apply to C2's almost exclusive confusion with C1, and to D2's preponderance of confusion with D1. It is only in the responses to A2 and D3 that a tendency to a complete semantic shift is evident. The other stimulus-response units mentioned above merely show an increased instability and variability. Thus the introduction of medium-sized N-figures tends to produce merely an extension of meaning of one of the names, while the introduction of the more distinctive size-1 or size-4 figures tend to produce a complete sematic change in one of the names and a compensatory extension of meaning of another.

The *associations* in Period II show little change as compared with Period I, and tables showing these responses have therefore been omitted. The chief change that occurs is a considerable shortening of the association times for all S's and an increasing tendency for the response 'D2' to be elicited by those names from other shape categories which have no strong associations within their own categories (i.e., the names of the medium and large figures). All S's showed a tendency in Period I to call D3 by the name of D2, and this tendency is greatly strengthened in Period II by the introduction of *D4. Thus 'D2' comes to be one of the most frequently elicited responses of the entire system, and apparently in consequence of this fact it is more likely than any other name to occur as an associative response to any name which has no strong association within its own category.

The results of Period II may be summarized as follows: given a series of objects of similar shape but of four different sizes, for any three of which names have been learned, the subsequent introduction of the previously absent size will have the following effects:

1. Size *1,* newly occurring, will elicit the name of *2,* and there will be an accompanying tendency, increasing with practice, for *2* to elicit the name of *3.*

2. Size *4*, newly occurring, will elicit the name of *3*, and *3* will increasingly tend to elicit the name of *2*.

3. Size *2*, newly occurring, will elicit from some individuals chiefly the name of *1* and from others chiefly that of *3*, but the latter tendency seems to be about twice as strong as the former.

4. Size *3*, newly occurring, will elicit chiefly the name of *2*, but from a few individuals chiefly that of *4*.

5. Thus where the newly introduced object is *intermediate* in size between two others, it elicits chiefly the name of that one of the two for which the responses previously were most *unstable*.

6. If we represent the names of the various sizes of any shape category by the letters *a, b, c,* then we may represent the equilibrium which such a system tends to approach by the pattern *abbc;* that is, if for four sizes only three names are available, such shifts occur as to give sizes 1 and 4 distinctive names, while sizes 2 and 3 elicit a common name. Further, responses *a* and *c* will have shorter reaction-times and be more stable then will the *b* responses. These relationships hold regardless of which of the four sizes was absent during the original learning of the names.

7. The name *b* is much more likely to occur as an associative response to names belonging to other categories than are the names *a* or *c*.

8. The above-mentioned tendencies are in general facilitated when (in Period II-*b*) the figures are presented in pairs, so that each figure appears many times alongside each of the others. The absolute values of the reaction-times are reduced, but the relationships between them remain the same. The variability in the responses to the intermediate figures increases markedly, while in the responses to the size-1 and size-4 figures it increases only slightly or even decreases, the latter being true for *A1 and *D4.

Individual Differences

The graphs of Figure 2 show the rate at which each S learned to name correctly the 12 O-figures. These graphs represent the learning up to the point where the S for the first time named all of the figures correctly in *one* R-series. The graphs of Figure 3 represent the further performance of each S during Period I; the last day of the graphs of Figure 2 is the first day of those in Figure 3. Figure 3 displays the degree of retention of the material over a period varying for the different S's between 122 and 135 days. The re-

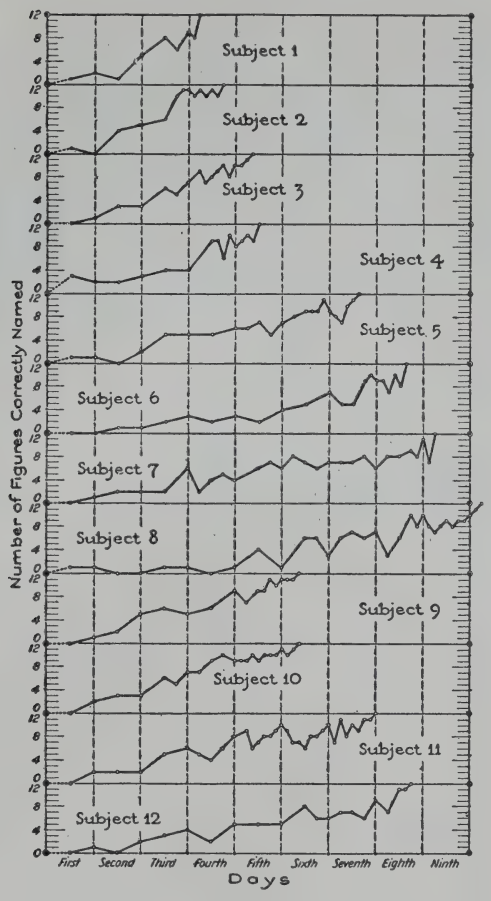

FIGURE 2

quirement during this period was that the S name all of the figures correctly in four successive R-series. The units of the horizontal axes represent days; each of the short vertical lines of uniform length *below* the horizontal axes indicates a sitting. The height of the vertical lines *above* the horizontal axes indicates the number of errors made (i.e., the number of *different* figures incorrectly named at a given sitting). The numbers placed in circles above these vertical lines indicate how many L-series were required before the S could on that day name all of the figures correctly in four successive R-

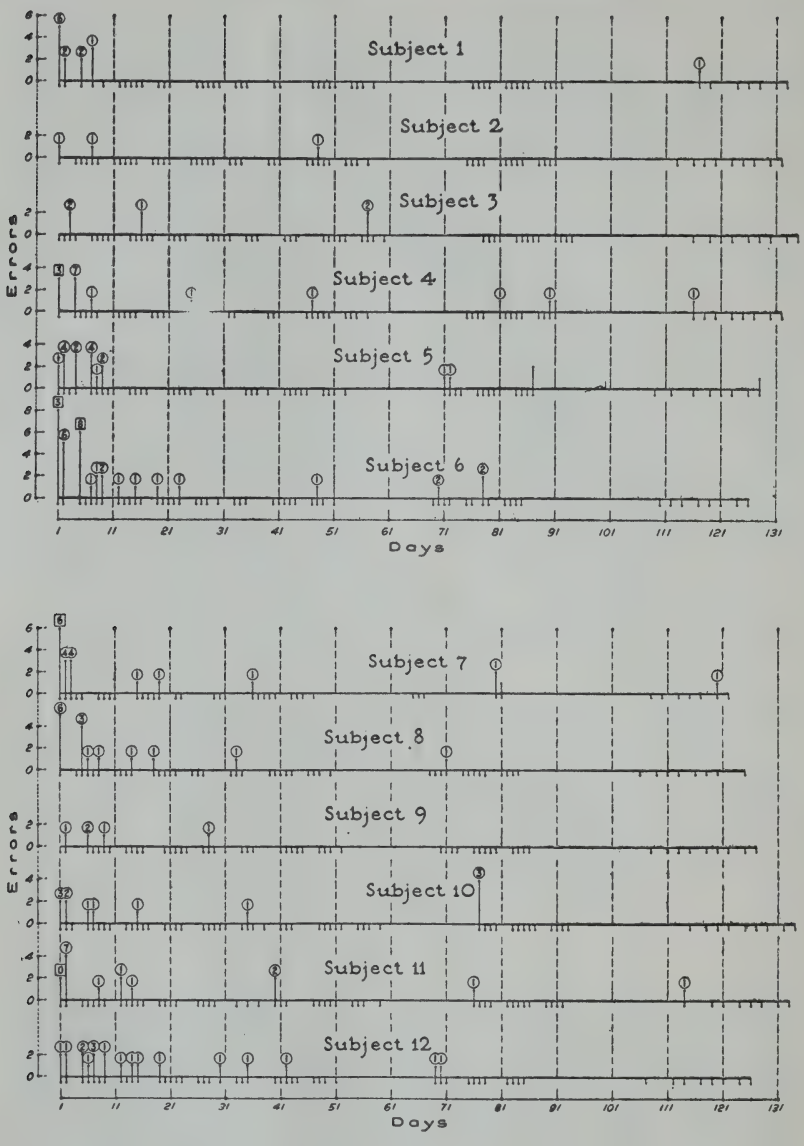
Subject 1
Subject 2
Subject 3
Subject 4
Subject 5
Subject 6
Subject 7
Subject 8
Subject 9
Subject 10
Subject 11
Subject 12
Errors
Days
1
11
21
31
41
51
61
71
81
91
101
111
121
131

FIGURE 3

TABLE 4

Summaries of Data for Individual Subjects, Period I

The vertical columns contain the following data: *S*: the number designation of each S; *L*: number of L-series required to attain 4 successive errorless R-series (all columns to the right of the *L*-column give measurements of performance *subsequent* to the attainment of this criterion); *Er*: total errors; *F*: failures to respond; *NRT*: average reaction-time in naming the nonsense figures; *NAT*: average association time in response to the figure names; *CNA*: total number of responses in the nonsense association tests which were common to at least 3 S's; *NAS*: nonsense association scatter (total number of *different* associative responses); *ERT*: average reaction-time in naming familiar objects in English; *EAT*: average association time for English words; *CEA*: number of English associations which corresponded to the most frequent responses of the group; *EAS*: English association scatter.

S	L	Er	F	NRT	NAT	CNA	NAS	ERT	EAT	CEA	EAS
1	32	20	7	93	109	66	19	68	90	68	14
2	31	12	0	109	102	63	21	68	90	53	16
3	35	16	1	88	97	56	24	60	71	68	13
4	46	10	2	103	122	69	22	84	94	71	13
5	54	23	6	92	—	0	—	70	124	47	22
6	70	30	9	92	323	21	49	54	168	35	23
7	76	45	2	80	93	69	20	66	71	71	13
8	99	13	4	118	129	66	28	79	93	57	18
9	43	34	0	87	122	54	34	86	113	64	16
10	46	12	3	98	—	0	—	77	202	24	46
11	63	16	2	114	176	22	56	91	180	32	35
12	63	28	10	100	131	35	48	88	125	55	21

TABLE 5

Responses to N-figures, Individual Subjects

In the vertical column on the left the S's are indicated by number. In the top horizontal row are indicated the N-figures (starred). Under *B2, e.g., Subject No. 1 has an average reaction-time in naming this figure of 1.26 sec.; in 8% of the presentations she calls it 'B1,' in 92% she calls it 'B3.'

	*A1		*B2			*C3			*D4	
S	RT	'A2'	RT	'B1'	'B3'	RT	'C2'	'C4'	RT	'D3'
1	117	100	126	8	92	90	100	0	122	100
2	88	99	89	5	93	111	99	0	75	99
3	77	99	91	88	11	68	100	0	81	99
4	77	99	94	9	91	82	100	0	80	100
5	70	99	83	46	54	82	88	12	87	97
6	85	99	94	87	11	87	97	3	87	99
7	70	100	75	15	85	72	95	4	80	100
8	103	99	116	6	93	122	95	3	106	97
9	77	100	105	93	7	82	99	1	82	94
10	73	99	95	6	93	101	23	76	86	99
11	102	98	108	3	96	115	99	0	109	95
12	84	96	95	3	92	105	92	2	106	99

TABLE 6

SUMMARIES OF DATA FOR INDIVIDUAL SUBJECTS, PERIOD II (O-FIGURES ONLY)

Under *RT* are given the average reaction-times of the various S's in naming the nonsense figures. Under *Errors* are indicated the total number of errors for all figures taken together, and the number of errors made in response to figures A2 and D3. All frequencies are in terms of number per 100 presentations. Under *Association* are given the number of associative responses common to at least 3 S's, and the average association times. No association measurements were made for S 7 in Period II.

S	RT	Errors			Association	
		Total	A2	D3	Com.	Time
1	88	46	12	21	60	100
2	90	74	13	40	60	75
3	86	37	3	5	44	76
4	89	139	54	74	65	98
5	79	82	10	32	0	—
6	84	36	2	3	15	288
7	76	198	71	63	—	—
8	108	41	4	7	57	95
9	78	30	2	4	46	96
10	89	24	3	2	4	215
11	117	96	47	14	23	128
12	88	31	8	2	21	105

series. Where the numbers are enclosed in squares, this means that the S, even after the indicated number of L-series, still could not give four correct R-series. The error lines without numbers represent errors made during reaction-time measurements. The two periods of about 20 days each in which no sittings are indicated were the Christmas and inter-semester vacations.

Table 4 summarizes the data for the individual S's in Period I. The error frequencies of Table 4 are based on an equal number of sittings (47) for all of the subjects. Tables 5 and 6 summarize the data for the individual S's in Period II. The relationships found in the data of Tables 4 to 6 are discussed under the following headings:

1. *Learning and Retention.* Both the graphs and the data of Table 4 show very considerable differences in rate of learning and in subsequent retention. There is some indication of a positive relation between learning and retention, although the experiment is not well designed for showing this relation, not only because of the (*necessarily*) small number of subjects but also because by the learning method used there was much overlearning of most of the material while the last one or two figures were being learned. Between

number of repetitions required for *one* correct R-series and number of errors and failures in the subsequent sittings of Period I, r=.54.[7]

2. *English and Nonsense Verbal Responses.* Between the reaction-times for English naming of familar objects and those for naming the nonsense figures, r=.52. Between the reaction-times for English associations and those for nonsense associations, r=.94. Between commonality of English associative responses and commonality of nonsense associative responses, r=.84 (i.e., those S's who gave the largest number of English associations which corresponded to the most frequent associations of the group did so also for the nonsense associations). Between English association scatter (number of different responses made to the same stimulus words on different occasions) and nonsense association scatter, r=.82. These results suggest that the same individuals who have developed the most specific and promptly released associations in ordinary language are also most likely to develop such associations in a new set of verbal material; those whose English associations are the most common associations of a group will also develop nonsense associations of this type. It should be noted that the most frequent English associations of this small group of S's were the same as those of a group of 100 subjects of an earlier study (1). The results also give us some confidence that, however artificial may be the linguistic situation provided by the experiment, the same processes of associative organization are at work here as in actual language.

3. *Association.* Between nonsense association time and nonsense association scatter, r=.85. Between time and commonality of associations, r=—.69. That is, the greater the variability in associative responses to the same words, and the fewer the responses which are most common in the group, the longer is the associative reaction-time. The correlations between *English* association time and *nonsense* scatter and commonality, and between *nonsense* association time and *English* scatter and commonality are almost identical with the above values. In the English associations, the r between time and scatter is .91; between time and commonality, —.81. These results are in

[7]All correlations are by the rank-difference method. While the reliability of any correlation based on 12 individuals is dubious, it will be seen in what follows that there are a number of relationships between various types of measurements made throughout the experiment which mutually confirm one another, and these consistencies may give some assurance that the relationships are not accidental. At any rate, important theoretical principles are suggested by the indicated relationships, and these principles will be tested in later numbers of these *Studies.*

agreement with the earlier study of the English associations of 100 subjects.

4. *Errors.* The errors made by the subjects in the course of this experiment are to be assigned a significance somewhat different from that assigned to errors in ordinary learning experiments. The errors here are variations of response which after the first weeks of learning are almost invariably of a systematic nature. The S tends to respond to a figure, not always with its own specific name, but sometimes with the name of the figure of the size larger or smaller, but of the same shape. This variation of response, this readiness to respond to an object not only with a uniform verbal response but sometimes also with the responses usually made to somewhat similar objects, undoubtedly is the behavior tendency which has been responsible for most linguistic (and logical) organization. Its end-products in language are semantic and phonetic word groups. 'Errors' of this type, particularly the semantic shifts which follow the introduction of the N-figures in Period II, represent a progressive change in behavior and *interconnection of response units* which are to be regarded as a very important form of human *learning.*

In Period I, those S's with the shortest reaction-times in naming the figures tended to make the largest number of errors (r=.74). Most of the responses counted as errors in Period I were 'multiple responses'; that is, the S on seeing a figure called out one name and immediately thereafter another, sometimes following this second name with a repetition of the first. Frequently the pronunciation of the first name was not completed before the S switched to the second. Either the first or the second response might be the previously learned name of the figure, though usually the second response was 'correct.' There was a strong tendency for certain of the figures to set off such multiple responses. These figures therefore tend to release two different responses, the one usually more readily or frequently than the other. But the recorded cases of such multiple responses were necessarily *overt* responses; nothing is more likely than that this simultaneous or successive activation of two response mechanisms occurred in the majority of the presentations of certain of the figures, at least for some of the subjects, but that they occurred *implicitly* preceding the overt and 'correct' response. The strong correlation between errors and reaction-time suggests that those S's whose overt responses were most prompt were most likely to give overt multiple or variable responses.

No significant relationship appears between the number of *overt*

errors made in Period I and the number made in Period II, nor between the total number of errors made in II and reaction-time. These facts reflect a much changed situation in Period II. The essential feature here is a difference in behavior of figures A2 and D3 as compared with the other figures. Certain S's, upon the introduction of the N-figures, not only give *A1 and *D4 the names previously learned for A2 and D3, but tend more and more frequently *to give A2 and D3 the names previously learned for A3 and D2.* By the last weeks of the experiment these S's invariably thus name A2 and D3, no longer as part of a multiple response, but as a prompt specific response to these figures. This renaming of A2 and D3 will hereafter be referred to as the *A2-D3 shift.* This shift, as we have seen, brings categories A and D into consistency with the other categories, so that the names of the four sizes of each category now conform to the pattern *abbc.* Other S's show this tendency to only a slight degree or not at all, making no more variable responses to A2 and D3 than to the other figures. Some S's who made very few overt errors in Period I show this tendency very strongly. Thus S 4, who made the smallest number of errors in Period I, was one of three S's making the largest number in II; but almost all of her errors in II consisted in the A2-D3 shift.

Since not all of the S's completed the association measurements of Period II, we can make comparisons only between the association measurements of I and the errors of I and II. We find between number of failures to respond in I and association time an r of .68. In so far as association time is a measure of associative organization, it therefore appears that those S's who most frequently are unable to make any verbal response whatever to an object show the least tendency toward such organization; those S's who show this tendency more strongly are more likely to respond with 'errors' than to make no response whatever. However, there appears no significant relation between *overt* errors in I and the association measures. Nor does such relationship appear in II, *except in the responses to A2 and D3.* Here those S's who show the strongest tendency toward the A2-D3 shift tend to have the shortest association times (r=.54), greatest commonality of associations (r=.75), and least associative scatter (r=.64). They also respond with the shortest reaction-times to the N-figures (r=.65) and they most consistently give these figures the names most frequently given them by the group as a whole (r=.51 in II-*a* and .83 in II-*b*). This last relationship is of especial interest. We have seen that the condition of maximum equilibrium

toward which each shape category tended after the introduction of the N-figures is that represented by the pattern *abbc*. To attain this equilibrium in categories A and D the A2-D3 shift was necessary; that is, a renaming of these two *old* figures. But in categories B and C this pattern was attained by an appropriate naming of the *new* figures, e.g., by giving *B2 the name 'B3' rather than the name 'B1.' It now appears that the S's who met the one condition tended also to be the ones who met the other, and also to be the individuals showing the greatest speed, specificity, and commonality in their associative responses *in the period preceding the introduction of the N-figures.* The differences between the individual S's in the tendency to the A2-D3 shift were much greater than in the commonality of their responses to the N-figures.

Further evidence of the differences among the individuals in their manner of adjustment to the new situation presented by the introduction of the N-figures is afforded by a comparison of their reaction-times in naming the O-figures in Periods I and II-*a*. The average reaction-times of some S's increased in II-*a*, those of others decreased. The following relationships appear: the smaller the percentage that an S's reaction-time in II-*a* was of that in I, the shorter was his average association time in I (r=.59), the greater the commonality of his nonsense associations (r=.75), the less his associative scatter (r=.67), the shorter his reaction-times for the N-figures (r=.62), and the greater his tendency to the A2-D3 shift (r=.62). And finally, we find that the shorter the reaction-time in naming the N-figures in II-*a*, the greater is the commonality of associations (r=.50) and the less the scatter (r=.51) in the period preceding the introduction of the N-figures.

The Relations between Instability and Association

We have seen that the most frequent type of 'error' in Period I was the 'multiple response'; the S begins with one response but 'corrects' himself, so that there is a pause between the two utterances, or at least a boundary produced by a strong stress on the corrected utterance. Either the first or the second utterance may actually conform to the conventional (originally taught) name for the figure. Such multiple responses can hardly be called contaminations, and yet the assumption seems justified that they are manifestations of the same underlying condition, viz., a state of balance between two reaction mechanisms. And, indeed, occasional examples of true contaminations occurred in responses to those figures which

tended most strongly to elicit multiple responses, e.g., figure B3. Had the verbal material been *dissyllabic,* and particularly if, instead of constituting a totally suppletive system, it had provided some initial sound similarities,[8] the multiple responses would probably more frequently have taken the form of contaminations.

Secondly, we have seen that the associations elicited by the nonsense names were in accordance with the multiple response tendencies, and that certain of these associations were as constant and speedy, at least for some subjects, as were the associations elicited by English words especially chosen as being among the most constant in the language. We must now consider the relationship between the multiple response tendencies and the associative tendencies.

It has been sufficiently demonstrated in earlier studies that in English, as well as in other languages, certain words will elicit the same associations on the part of from 60 to 90% of any sufficiently large group of individuals, with very short average reaction-times. It is also known that these words have been involved in considerable analogic change in the histories of the various languages. And thus, in conformity with the psychological doctrines current in 19th century linguistics, the analogical changes have been causally attributed to the associations. If a word *a* is strongly associated with a word *b* (so runs the explanation), then the *idea* of *a* not only tends to arouse the corresponding articulation but also the *idea* of *b*, the latter tending also to arouse a corresponding articulation. The result will frequently be an *Entgleisung,* a 'derailment,' the articulation of *b* beginning before that of *a* has been completed. Or, to put the matter in more modern physiological terms, if the sound and articulation of a word *a* have become conditioned stimuli tending strongly to elicit word *b*, then the beginning of the articulation of *a* may activate the *b* mechanism so strongly that an interference of responses results.

There are serious weaknesses in this explanation. Chief among these is the fact that though certain associations are found to be almost universal at the present time, and very constant and prompt in most individuals, they yet show very little tendency to produce mixtures of articulation. And, on the other hand, very many of the momentary *lapsus linguae* which have been reported by various writers[9] do not involve words between which strong or common associations can be demonstrated. Secondly, it is extremely doubt-

[8] As in Experiment I and II of my monograph (2).
[9] E.g., Meringer and Mayer (5).

ful whether, if a verbal mechanism *b* must first be aroused by another verbal mechanism *a,* it can be set in operation so quickly as to interfere with the articulation of *a.* And, finally, it seems highly unlikely that whenever a situation calls out, e.g., the verbal response *black,* this word should automatically drag forth the word *white,* regardless of the nature of the stimulating situation.

These deficiencies in the traditional explanation seem to me to proceed from an almost total neglect of the relationship of verbal responses to the stimulus situations which elicit them, and of the fundamental question of how the verbal associations themselves come to be established.

Why do certain words associatively elicit specific other words? The explanation offered by one of Thumb's critics was that the subjects *auf Schulbänken gesessen haben.* And, indeed, this would seem to indicate one type of situation in which verbal associations might well be established. For so far as our knowledge of the conditions of learning goes, if a sound-sequence or articulation *a* is to elicit an articulation *b,* then *a* and *b* must have occurred simultaneously or successively a certain number of times. Obviously, in the rote memorizing of grammatical paradigms, arithmetic tables, etc., such conditions are met, and this must account for much of the uniformity which we find in the associations of most individuals. But linguistic change did not begin with the foundation of schools, nor is it absent among unschooled populations today. And I have not found the associations of uneducated individuals or young children to differ much from those of the educated, *when the stimulus-words are chosen from the more universal and common stock of the language.*

The more general principle would be that the stimuli resulting from one verbal response become capable of eliciting a specific other verbal response whenever there is a frequent recurrence of environmental situations which tend to elicit both responses simultaneously or successively. An environmental situation might elicit such multiple responses either because it contains *both* of the objects *a* and *b,* each of which tends to elicit its own specific response, or because it contains an object *a* which tends to elicit not only its own specific response but also the response specific to another (absent) object *b.* This last type of situation is the type occurring in this experiment, and it must be a type very frequent under ordinary conditions. For the specificity of stimulus objects and of responses is a relative matter; every object in the world has some characteristics in common

with other objects, and may on occasion elicit the verbal responses which are relatively specific to these other objects. Experimenters who work with 'nonsense' material know how impossible it is to obtain any pattern, however fantastic, which does not set off some conventional verbalization. Thus when we say that a certain response is the specific response to an object, we are really stating a statistical probability; given numerous occurrences of this object, this response is the one which will occur most frequently.

The above formulation would mean a reversal of the traditional explanation; it is not the associations which cause the variations in response, but the tendency to variability is responsible for the associations. Thus both mixtures of articulation and verbal associations proceed from a common cause: the tendency of certain stimulus-objects to elicit multiple responses. It will be true, of course, that if we find a word *a* closely associated with a word *b* on the part of many individuals, this may serve as evidence that certain stimulus situations have had a strong tendency to elicit simultaneously or successively the two responses, and that under certain additional conditions this may result in mixtures of articulation. But, as has been pointed out above, mixtures of articulation may occur between words which are not at all strongly associated; a certain stimulus situation occurs which for the first time in the individual's history activates two particular verbal mechanisms simultaneously, and a 'contamination' may result in the absence of any demonstrable previous association. A verbal association therefore tells us at most what verbal mechanisms have tended to be simultaneously activated most frequently in the past. But it does not tell us under what conditions such simultaneous activation will take the form of contamination or analogic change.

Two further points are suggested by the data of this experiment. First, certain subjects may show extreme variability in their associative responses and yet make many variable responses to the figures. That is, a certain number of joint activations of two response mechanisms may in one individual result in a firm interconnection between them, and in another individual produce hardly any such interconnection. And yet in general the individuals who show the greatest tendency toward variation of response show also the firmest associations.

Secondly, let us consider, e.g., the responses to figures C1 and C2. The association 'C1'→'C2' is extremely constant, speedy, and common to all S's, whereas the reverse association is weak. Does this

mean that the stimulus-response unit C1→'C1' is more unstable than the unit C2→'C2'? The error records contradict this; the latter unit is much more unstable, particularly in Period II-*b*. In other words, if a verbal association *a→b* is very strong, and the reverse association is weak, *then it is not* a *but* b *which is unstable.* Given an object *B* whose name is *b,* the frequent occurrence of *b* as an associative response to other words indicates not *necessarily* an instability in the relation of these other words to their objects, but an instability of the stimulus-response mechanism *B→b.*

Summary

The results of the present experiment suggest the following tentative principles:

1. Instability of verbal reactions results when stimulus-objects, because of common features, tend to activate not only the response most specific to them but also the responses relatively specific to other objects ('multiple responses').
2. This instability is revealed by frequent occurrences of the simultaneous or successive activation by one stimulus-object of two or more response mechanisms, and, where this tendency is strong, by lengthened reaction-time in the verbal response to the object.
3. This simultaneous activation of two or more response mechanisms may result in an interconnection between them, so that the sound or articulation corresponding to the one associatively elicits the other.
4. Verbal associations are possible *results* and indicators, not *causes,* of 'multiple response' tendencies such as are exemplified by contamination and analogic change.
5. A very strong verbal association *a→b,* when the reverse association *b→a* is weak, may indicate an instability of the response *b* in relation to its stimulus-object *B,* rather than an instability of *a* in relation to its stimulus-object *A.*
6. When a stimulus-object *B,* whose name is *b,* tends to be confused with *two* other objects *A* and *C* in about equal degree, the stimulus-response unit *B→b* may be very unstable, but the name *b* will elicit extremely variable associations, with long association times.
7. When to a group of named objects there is added a new, unnamed object resembling the others in such an attribute as shape, the new object elicits the name of that one of the original objects which most closely resembles it in other attributes (as of size);

or, if two of the original objects so resemble it equally, it elicits chiefly that verbal reaction which was previously most *unstable* and least *specific*. Such an introduction of a new object temporarily increases the instability of the entire system, but there is a tendency toward a new equilibrium, the pattern of which has been described in preceding pages.

8. Decided individual differences were found in the manner and degree of verbal organization. Those individuals whose reaction-times are shortest tend to make the most frequent *overt* multiple or variable responses in naming stimulus-objects, but long reaction-times may indicate frequent *implicit* responses of this type, which show their effect in the associative connections between the words. In general, those individuals whose verbal associations show the greatest commonality with those of the group, the greatest constancy, and shortest associative reaction-time show the most consistent and ready tendencies toward organization of the verbal material, especially when this organization involves shiftings and changes in a system of stimulus-response units to restore a disturbed equilibrium. These facts very definitely suggest that the various individuals of a speech community would participate in very different degrees in the initiation of semantic and analogic change.

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ÉTUDE DANS L'ORGANISATION DU COMPORTEMENT LINGUISTIQUE: I. LES CARACTÉRISTIQUES DES RÉACTIONS VERBALES INSTABLES

(Résumé)

Les conditions fournies dans cette expérience ont été les suivantes: (*a*) on a joint des réponses verbales (syllabes non-sens) à nombre d'objets stimuli (figures non-sens) se ressemblant en forme ou grandeur, mais choisis de sorte qu'il y a eu une certaine asymétrie dans les classes objectives auxquelles ils appartiennent: et (*b*) on a introduit après d'autres objets ressemblant systématiquement aux premiers objets mais pour lesquels aucuns noms spécifiques n'avaient été appris. Les résultats suggèrent les principes suivants: (1) L'instabilité des réactions verbales résulte quand les objets stimuli, à cause de traits communs, tendent à activer non seulement la réponse la plus spécifique à eux-mêmes mais aussi les réponses relativement spécifiques à d'autres objets ('réponses multiples'). (2) Cette instabilité est révélée par des occurrences fréquentes de l'activation simultanée ou successive par un objet-stimulus de deux ou plus mécanismes de réponse, et où cette tendance est forte; par un plus long temps de réaction de la réponse verbale à l'objet. (3) Cette activation simultanée de deux ou plus mécanismes de réponse peut résulter en un rapport réciproque entre eux, de sorte que le son ou articulation correspondant à l'un évoque l'autre. (4) Les associations verbales sont des *résultats* possibles et indicatrices, non pas des *causes,* des tendances de 'réponse multiple,' telles que montrent par exemple la contamination et le changement analogique. (5) Les personnes dont les associations verbales montrent la plus grande ressemblance à celles du groupe, la plus grande constance, et le plus court temps de réaction aux associations montrent les tendances les plus constantes à l'organisation du materiel verbal, surtout quand cette organisation comprend des déplacements et des changements dans un système d'unités de stimulus-réponse pour restaurer un équilibre dérangé.

ESPER

UNTERSUCHUNGEN AN DER ORGANISATION DER SPRACHTÄTIGKEIT: I. DIE BESCHAFFENHEIT LABILER SPRACHREAKTIONEN

(Referat)

Die in dieser Untersuchung verschafften Bedingungen waren wie folgt: (*a*) Sprachreaktionen (sinnlose Silben) wurden mit einer Anzahl von Reizgegenständen verbunden [attached] welche einander in Bezug auf Form oder Grösse ähnlich waren, welche aber so ausgelesen worden warden, dass in Bezug auf die objektiven Klassen, denen sie angehörten, eine gewisse Assymetrie bestand; und (*b*) es wurden später weitere Gegenstände eingeführt, die den ursprünglichen Gegenständen systematisch glichen, für die aber keine spezifische Namen gelernt worden waren. Die Befunde weisen auf folgende Grundsätze hin: (1) Es wird Unstetigkeit der Sprachreaktionen verursacht, wenn Reizgegestände, infolge gemeinsamer Eigenschaften, dazu neigen, nicht nur die ihnen spezifischste Reaktion, sondern auch die, für andere Gegenstände relativ spezifischen Reaktionen ('multiple Reaktionen')

in Gang zu setzen. (2) Diese Unstetigkeit offenbart sich im häufigen Vorkommen simultaner oder aufeinanderfolgender Aktivierung zweier oder mehrerer Reaktionsmechanism, und, wo diese Neigung eine starke ist, in einer Verlängerung der Reaktionszeit der Sprachreaktion auf den Gegenstand. (3) Diese simultane Aktivierung zweier oder mehrerer Reaktionsmechanismen kann eine gegenseitige Verbindung [interconnection] verursachen, so dass der dem einen Reaktionsmechanismus entsprechende Laut oder die entsprechende Ausprache [articulation] nun assoziativ den anderen Mechanismus hervorruft. (4) Sprachassoziationen sind mögliche *Resultate* und Andeutungen, nicht *Ursachen* der Neigung zu multiplen Reaktionen wie sie sich zum Beispiel in der 'Verunreinugung' (einer Assoziation durch eine andere) [contamination] und in analogischen Veränderungen [analogic change] erweist. (5) Jene Individuen, deren Assoziationen die grösste Gemeinschaft mit denen der Gruppe als Ganze, die grösste Beständigkeit, und die kürzeste assoziative Reaktionszeit erweisen, erweisen auch die beständigsten Neigungen zur Organisierung des Sprachstoffes, besonders wenn diese Organisierung Verschiebungen und Änderungen eines Systems der Reiz-reaktionseinheiten zur Wiederherstellung eines gestörten gleichgewichts in anspruch nimmt.

ESPER

AN APPARENT TRANSFER OF CONDITIONING*

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INTRODUCTION

Pavlov (1) reports two dynamically opposite types of phenomena which may develop when an extra stimulus is repeatedly presented in combination with a firmly established and well-reinforced conditioned stimulus and the combination is never reinforced by the underlying unconditioned stimulus. The one is a positive or excitatory tendency in which the extra stimulus acquires the response of the conditioned one. This he terms a *second-order conditioned reflex* (p. 33). The other is a negative or inhibitory tendency in which the presence of the extra stimulus in the combination reduces or inhibits the response to the conditioned stimulus which when presented alone still evokes a strong response. This he terms *conditioned inhibition* (pp. 68 ff.).

According to him (p. 33), the determining factor in the development of these phenomena is the extent of the interval between withdrawal of the extra stimulus and presentation of the primary conditioned one. With a long interval, conditioning of the second order develops; with a shorter one, conditioned inhibition develops. He reports an experiment by Dr. Kasherininova (p. 72) in which the presence of the extra stimulus did produce a strong inhibitory effect on the conditioned one, but at the same time the extra stimulus acquired some of the excitatory properties of the conditioned one, so that when used alone it behaved as a conditioned stimulus of the second order. In this connection he states:

> ". . . . it is quite possible that a transitory acquisition of weak excitatory properties of the second order is of no infrequent occurrence in the development of conditioned inhibition."

But, regardless of the extent of the interval between the extra

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[1]The writer wishes to express his appreciation to Professor Clark L. Hull under whose direction this investigation was carried out.

and primary conditioned stimulus provided it is not too great to permit a functional connection between the two, it does not seem probable that the presentation of the two together always without reinforcement can eventually lead to anything but complete inhibition. It seems quite likely that all conditioning of the second order which is set up subsequent to establishment of the requisite conditioning of the first order is only a transitory phase of conditioned inhibition.

Because of this likelihood of second-order conditioning eventually terminating in complete inhibition upon repeated reinforcement, a method permitting indefinite reinforcement whereby an extra stimulus could be made to acquire the excitatory properties of a conditioned one was sought. The one such possibility which suggested itself was that of setting up the connection between the extra stimulus and the one which was to become the first-order conditioned stimulus, before establishing the connection between the first-order and unconditioned stimuli. Such a procedure seemed to allow for indefinite reinforcement of the connection between the extra stimulus and that which was subsequently to become the first-order conditioned one, provided this reinforcement was carried out prior to the primary conditioning. Should an excitatory tendency become set up in this way, it was felt that it would represent a unique type of conditioned reaction, since through the process of conditioning a stimulus would be made to evoke a response with which it had never been presented in temporal contiguity.

However, a review of the literature revealed that this idea was not entirely new. An experiment was found reported by Prokofiev and Zeliony (2) in which conditioned reflexes were established in a similar way. Here human subjects were presented with two supposedly neutral stimuli which were given together a number of times. Following this, one of these stimuli was conditioned to a reaction and the remaining one was then tested to determine if the conditioning had been extended to it. These two neutral stimuli were the sound of a metronome and a rhythmic tactile stimulation which were allowed to continue for 10 seconds. After presentation of this combination from 5 to 10 times per day for several days, the tactile stimulation, without the sound of the metronome, was given together with an electric shock to the hand which immediately followed it and served to evoke a reflex retrogression of the arm. After this combination had been given a number of times the

metronome was sounded alone and found to produce a similar retrogression of the arm. The number of subjects employed is not stated.

A similar experiment reported in the same article was carried out on a dog. A conditioned salivary reflex was established in the customary Pavlovian manner by sounding a metronome just before introducing hydrochloric acid into the mouth. This conditioned reflex having been established, the introduction of the acid into the mouth (without the sounding of the metronome) was then followed immediately by an electric shock on the foot, evoking a foot-withdrawal. This combination was presented a sufficient number of times firmly to establish a conditioned foot-withdrawal reaction. The subsequent sounding of the metronome, however, did not result in a withdrawal of the foot.

The extreme duration of the two "conditioned" stimuli in the above experiment on humans renders doubtful the true conditioned nature of the response. The 10 seconds during which the metronome was allowed to sound would allow ample time for the subject to consider whether or not it would be advisable to withdraw his hand on the chance that the shock might be given. Such a deliberated type of response would hardly be what is commonly considered a conditioned reflex.

The present experiment was designed to determine whether or not this new type of conditioned response could be set up in humans if the duration of and intervals between the respective stimuli were sufficiently reduced to eliminate the possibility of deliberation on the part of the subjects. To this end a faradic shock on the pad of the right index finger was used as the unconditioned stimulus for evoking a withdrawal of the finger. A blow on the cheek just below the right eye serving in itself to evoke a wink, and which will hereafter be referred to as the strike, was used as the primary conditioned stimulus. A very faint flash of an electric light was used as the extra, and, therefore, critical stimulus. The specific problem was to determine whether, by presenting the flash with the strike a number of times and then following this by an establishment of conditioning between the strike and withdrawal of the finger, the flash could be made to produce withdrawal of the finger.

Apparatus, Subjects, and Instructions

The subject, seated in a slightly reclining position in a Morris chair, was surrounded from the waist up by a blackened hood

which served the threefold purpose of (1) diminishing the visibility of the padded hammer used to evoke the wink, (2) reducing eye strain, and (3) preventing any visual cues that might unintentionally be given by the experimenter. To further these effects the room was somewhat darkened by drawing the shade.

A small electric light dimmed by a layer of black sateen so as to produce at most only a very minimal wink, fastened inside the hood approximately 2 feet directly in front of and on a level with the eyes of the subject, served as the extra stimulus.

For eliciting the unconditioned wink a spring-driven padded hammer provided with an electromagnetic release was allowed to produce a sudden rap on the right cheek just below the eye. This was similar in all respects to the hammer used by Switzer (3) for the same purpose. The hammer and releasing mechanism, which were fastened inside the hood, were blackened so as to reduce their visibility. Cocking of the hammer was accomplished by means of a thread pulled manually by the experimenter.

Recording of the winks was accomplished by a finely drawn glass needle in contact with the horizontally running smoked paper band on an extension kymograph. The glass needle was activated by a fine silk thread which was run from its point of attachment on the needle through a piece of bent glass tubing and down through a small slot in the top of the hood, and then fastened to the eyelid of the subject by a bit of adhesive cement. Tension on the thread was regulated by horizontal adjustment of the needle.[2]

For eliciting withdrawal of the finger a tetanizing faradic current from an inductorium in an adjoining room was delivered to the pad of the subject's right index finger, the more diffuse electrode being applied to the wrist. The intensity of the shock was regulated by adjusting the secondary coil of the inductorium. In all cases an intensity of shock was used which was sufficient to insure a marked finger-withdrawal. This had to be regulated for each subject, but in practically all cases a well-marked finger-withdrawal was maintained throughout the series beginning not later than the fifth shock.

Recording of the finger movement was accomplished by means of a recording ammeter. This was constructed from a small-sized D-C ammeter, the indicator of which was clipped off leaving only a

[2]The winks were disregarded in the treatment of the results.

small stub to which a celophane stylus was attached. The ammeter was fastened in an inverted position to enable the stylus to record on the horizontal band. The pad of the subject's right index finger lay on a small lever activating a telephone resistance button in circuit with the recording ammeter and an auxiliary storage battery. The lever, which was subjected to an upward pressure by a coiled spring was adjusted so that when held down it exerted a pressure on the resistance button. Any upward movement of the finger, therefore, relieved the pressure on the button and produced a corresponding deflection of the stylus recording on the band. This method of recording proved to be extremely sensitive. As the extent of the resulting tracing could not be considered a very direct measure of either the direction or extent of finger movement, these records were treated in a qualitative way only. Any deflection on the record was considered a response, and no distinctions were made between the amplitudes of these deflections.

Recording of the various stimulus combinations was accomplished by the use of electromagnetic signal markers placed in parallel circuit with the various stimulating devices. The current used to operate the stimulating devices and their markers was taken from a 6-volt storage battery.

The use of a constant hum presented through head phones in circuit with an A-C transformer served as a masking sound to eliminate auditory cues.

The stimuli were activated automatically at regular intervals of 38.5 seconds by adjustable motor-driven switches in an adjoining room. The switches were so adjusted that the interval between the component stimuli of a double stimulation was approximately .4 seconds. Post-experimental photographic determination show that where the flash-strike combination was used the interval between the onset of the flash and that of the strike was .43 second, the duration of the flash being .26 second and the interval between the termination of the flash and onset of the strike .17 second. Where the strike-shock combination was used the interval between the onset of the strike and that of the shock was .42 second. These temporal relations were undoubtedly subject to some degree of variation due to wearing of the contact points and slight errors in their adjustment. The components of any stimulation were determined before automatic completion of the circuit at the regular 38.5-second interval had taken place. This was effected by means of switches operated by the experimenter in the room with the subject.

The subjects were all male college students. They were instructed at the beginning of each experimental period to keep their fingers on the button, their eyes open and looking straight ahead, and to hold their heads as still as possible.

Procedure and Results

Three groups of subjects, an experimental group, A, and two control groups, B and C, were used. Each was put through three experimental periods, one on each of three successive days. The order of stimulation for Group A, in which the interval between the two components of a double stimulation was approximately .4 second and that between successive stimulations was 38.5 seconds, was as follows:

First day

5 successive presentations of the flash alone.
15 " " " " flash and strike.
5 " " " " strike alone.
10 " " " " flash and strike.

Second day

15 successive presentations of the flash and strike.
10 " " " " strike alone.
5 " " " " flash and strike.

Third day

5 successive presentations of the flash and strike.
25 " " " " strike and shock.
2 " " " " flash alone.

It will be seen that the first two periods were given over to what may be considered the establishment of conditioning between the flash and the response to the strike. This was simply assumed; no test for it was given. On the third day, after five successive reinforcements of this conditioning, the strike, or its response, was conditioned to the finger-withdrawal reaction. This conditioning was also assumed; no test for it was given. If, then, when presented subsequent to this conditioning the flash evoked a finger reaction at either of the two presentations this was considered evidence of either this novel type of conditioning or some such generalizing factor as irradiation. Fifteen subjects were put through this procedure. Of these, 9 gave one or more positive responses to the flash at the end of the third period.

To ascertain whether or not these responses were genuine cases of conditioning rather than the result of some process such as irradia-

tion, Control Groups B and C were run. Group B, which consisted of 10 subjects, was put through exactly the same procedure as Group A with the one exception that wherever Group A was given the flash-strike combination Group B was given the strike alone. In other words, in this group there was no establishment of conditioning between the flash and the wink. Group C, which consisted of 11 subjects, was likewise put through exactly the same procedure as Group A with the one exception that wherever Group A was given the strike-shock combination Group C was given the shock alone. In this group there was consequently no establishment of conditioning between the strike or its response and finger-withdrawal. Table 1 permits a view of the procedures and results of the three groups as a whole.

Thus in Group B there was no establishment of the conditioning between the extra and conditioned stimuli. Likewise, in Group C there was no establishment of what in the case of Group A would be considered the conditioning between the first-order and unconditioned stimuli. As not a single subject in Group B and only one in Group C gave a response to the flash at the test point at the end of the third day, it seems evident that it is necessary for both these conditionings to be established for this response to the extra stimulus to occur with any degree of regularity.

TABLE 1

SHOWING THE SUCCESSIVE STEPS IN THE PROCEDURES AND THE PERCENTAGE OF SUBJECTS RESPONDING AT THE TEST POINT FOR EACH OF THE THREE GROUPS

Group A (15 subjects, 3 sessions)	
Step 1.	flash and strike
" 2.	strike and shock
" 3.	flash → finger-withdrawal (*60%* subjects)
Group B (10 subjects, 3 sessions)	
Step 1.	strike
" 2.	strike and shock
" 3.	flash → finger-withdrawal (*0%* subjects)
Group C (11 subjects, 3 sessions)	
Step 1.	flash and strike
" 2.	shock
" 3.	flash → finger-withdrawal (*9%* subjects)

Interpretation

In Group A an appreciable number of subjects, 60%, responded to the extra stimulus at the critical point by withdrawing the finger. That this occurrence cannot have been due to irradiation or a lowering of the response threshold to stimuli in general is brought out by the completely negative results of Group B. That it must have been dependent upon the conditioning of the critical response is brought out by the practically negative results of Group C. This therefore must be regarded as a genuine conditioned reaction. Furthermore, since the flash or extra stimulus had never before been presented with the withdrawal of the finger, it would appear, on the surface at least, that this conditioning had been established in the absence of temporal contiguity between the two. Three mechanisms suggest themselves in possible explanation of this phenomenon:

1. Through having been presented in temporal contiguity with the strike, the flash may in a sense have become its functional equivalent and, as such, become capable of evoking its conditioned reaction. If this were so the phenomenon could be conceived as a "pre-established" second-order conditioned reaction.

2. The reaction may have been the result of a "chaining" process. It will be recalled that in the first step of the procedure the flash was always accompanied by the wink, in the second step the wink was always accompanied by the shock, and in both steps conditioning was given sufficient opportunity to take place. In view of the common bond between the two steps it is possible that the flash may have evoked its original conditioned response, the wink, the kinaesthesis from which in turn served as the conditioned stimulus evoking withdrawal of the finger. Such a phenomenon could aptly be called a "conditioned chain reaction." This interpretation would demand a relatively long latency for the reaction and the appearance on the record of conditioned winks immediately preceding the finger responses. Unfortunately, the slow speed at which the kymograph was run did not permit any analysis of these temporal relations.

3. The reaction may have been due to some factor of generalization inherent in the conditioning process itself. In other words, it is barely possible that once a stimulus has been conditioned to a given response its threshold for evoking other conditioned responses thereby becomes materially lowered. Such a possibility would be in harmony with the similar though dynamically opposed type of gen-

eralization described by Pavlov in connection with conditioned inhibition of "heterogeneous" conditioned reflexes (p. 77).

Summary

A very faint flash of an electric light was paired a number of times with a sudden tap on the cheek which served to evoke a wink. After conditioning between the two had been given sufficient opportunity for establishment the tap on the cheek was repeatedly paired with a shock evoking withdrawal of the finger. The subsequent presentation of the flash produced sudden withdrawals of the finger in 9 out of 15 subjects.

The employment of two control groups showed that the reaction did not occur with any appreciable degree of regularity unless conditioning was allowed to occur both between the flash and wink and between the wink and withdrawal of the finger.

This therefore appears to be a type of conditioned reaction which does not depend for its establishment upon temporal contiguity between the stimulus and the response with which it is to become connected.

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UN TRANSFERT ÉVIDENT DU CONDITIONNEMENT

(Résumé)

Un très faible jet de lumière d'une torche électrique a été associé plusieurs fois à un petit coup rapide sur la joue duquel l'on s'est servi pour évoquer un clin d'oeil. Après une durée suffisante pour établir le conditionnement entre les deux, le coup sur la joue a été associé plusieurs fois à un choc causant qu'on retire le doigt. La présentation subséquente du jet de lumière a fait retirer le doigt subitement à 9 sur 15 sujets.

L'emploi de deux groupes de contrôle a montré que la réaction n'a pas eu lieu avec un degré évident de régularité à moins que le conditionnement n'ait été permis d'avoir lieu et etre le jet de lumière et le clin d'oeil et entre le clin d'oeil le moment de retire le doigt.

On trouve donc ici ce qui paraît être un type de réaction conditionnelle qui ne dépend pas pour son établissement d'une contiguïté de temps entre le stimulus et la réponse à laquelle on doit l'associer.

SHIPLEY

EINE AUGENSCHEINLICHE ÜBERTRAGUNG DER BEDINGUNG

(Referat)

Ein sehr schwacher Strahl eines elektrischen Lichtes wurde mehrmals mit einem plötzlichen Taps auf die Backe, der ein Blinzeln [wink] hervorrief, gepaart. Nachdem genügend Gelegenheit zur Etablierung der Bedingung zwischen den zwei Reizen gegeben worden war, wurde der Taps auf die Backe wiederholt mit einem, Zurückziehung des Fingers verursachenden, Schock gepaart. Die nachfolgende Presentierung des Strahles verursacht bei 9 unter 15 Versuchspersonen plötzliche Zurückziehungen des Fingers.

Durch Verwendung zweier Kontrollgruppen wurde erwiesen, dass die Reaktion nicht mit bemerkbarer Regelmässigkeit stattfand wenn man die Bedingung nicht sowohl zwischen dem Strahl und dem Blinzeln wie zwischen dem Blinzeln und dem Zurückziehen des Fingers stattfinden liess.

Es scheint hier also eine Art der bedingten Reaktion zu bestehen, deren Etablierung nicht von dem zeitlichen Aneinandergrenzen [temporal contiguity] des Reizes mit der Reaktion, mit der er verbunden werden soll, abhängig ist.

SHIPLEY

THE FORMATION OF NEGATIVE CONDITIONED REFLEXES BY COINCIDENCE IN TIME WITH THE PROCESS OF DIFFERENTIAL INHIBITION*[1] [2]

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Since the early investigations on conditioned reflexes, a characteristic of these reflexes was seen to be a tendency to constant fluctuations, sometimes resulting in their complete disappearance. The instability of conditioned reflexes, discovered by the first investigators, was explained later on as a constant interruption in the reflexes by the process of inhibition. The tendency of conditioned reflexes to fluctuations caused by the slightest changes in the environment and, particularly, in the course of experimentation, is explained by the fact that, together with the process of excitation in the central nervous system, the opposite process of inhibition constantly sets in.

Recently it has been proved that regulated activity in the higher parts of the nervous system can proceed only if the two processes are well balanced; and this depends partly on other external conditions, leading to the dominance of one of these processes caused by the appearance of the stimulation.

It was noted from the very first investigation that inhibition may be caused in the presence of some constant, external stimulation or without it; and according to this principle the first classification of different kinds of inhibition was made. At present, the opinions on

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[1]Some of the experiments in the present communication were reported by G. W. Volborth at the Thirteenth International Physiological Congress (August, 1929, Boston, Massachusetts); a detailed description of the whole investigation is being published by the author in an article under the same title in the *Journal of the Ukrainian Psychoneurological Institute* in a series entitled "Conditioned Reflexes" (edited by G. W. Volborth and A. I. Heimanowitch). Only the principal data of this investigation are given in this paper.

[2]Sincere thanks are due to Professor G. W. Volborth for his valuable advice and suggestions during the performance of the experiments here reported.

this subject have changed; now we distinguish two kinds of inhibition. One is always functionally present in the conditioned reflex, always determining it in time as well as with regard to the other properties of the stimuli, depending upon the conditioned stimulus itself; as it is a special, functional connection with the center of the conditioned reflex, it is termed *internal inhibition.*

A second kind of inhibition, the so-called *external inhibition,* is caused by excitation in various other parts of the central nervous system, not specially connected with the conditioned reflex. At present all processes going on in the central nervous system during external inhibition, as well as during all the different forms of internal inhibition, sleep included, are considered essentially the same. The present paper is directly concerned with the group of internal inhibitions.

The first facts concerning inhibition of conditioned reflexes were obtained and elaborated by Babkin (2) and Tolochinoff (19); they observed that, after repeated application of the conditioned stimulus at short intervals without subsequent reinforcement by the unconditioned stimulus, the conditioned salivary reflex gradually decreases and finally disappears completely. This process was termed *extinction of conditioned reflexes* and was studied by Babkin (2), Kasherininova (8), Zeliony (23), Perelzweig (16), Podkopaev (17), and others. In the case of extinction without the action of any casual, additional stimuli, the conditioned stimulus gradually loses its exciting effect, and under the action of the same stimulus there is a change from excitation to the process of inhibition; but this loss of effect is not irrevocable—in the course of time the extinguished reflex is restored; this restoration can be accomplished more quickly by applying an unconditioned stimulus; an analysis of the restoration of extinguished reflexes is given by Podkopaev (17).

Another case of inhibition was specially studied by Zavadsky (22) who investigated delayed conditioned reflexes. In Zavadsky's experiments the duration of the isolated action of the conditioned stimulus (i.e., the time from the beginning of the action of the conditioned stimulus till the moment of reinforcement by the unconditioned stimulus) was from three to four minutes. It was observed that when the delayed conditioned stimulus began to act the flow of saliva did not commence at once, but only after some time (one or two minutes), thus approaching the moment when the unconditioned reflex began to act; that is, it was found that when the conditioned

stimulus was much delayed from the beginning of the action of the unconditioned stimulus, the former evoked salivary secretion only during the second part of its isolated action; on the contrary, during the first part, it stopped, i.e., inhibited, the secretion of saliva. This kind of inhibition is termed *delayed inhibition* (retardation).

The third kind of inhibition, *differential inhibition,* is caused by setting one single and definite conditioned stimulus, always reinforcing it, against another, proximate stimulus without reinforcing the latter by the unconditioned stimulus; here we observe that when a second stimulus without reinforcement is used the process of inhibition gradually develops in the same analyser [Zeliony (23), Nikiforovsky (13), Beliakov (3), Krasnogorsky (9), Gubergritz (7)]. These three processes of inhibition, not connected with the application of any new additional agents, were from the very first referred to as internal inhibition.

The question of *conditioned inhibition,* studied in detail by Vasiliev (20), Nikolaev (14), Mishtovt (12), Perelzweig (16), and others, is more complicated. This kind of inhibition occurs as follows: Inhibition is evoked by repeated coincidences in time of the action of the conditioned stimulus with an extraneous agent without supporting this combination by the unconditioned stimulus. Thus there is a conditioned stimulus always supported, and a combination of the same stimulus with an additional agent, which is never supported. Since an additional agent was present in the process of conditioned inhibition, this type of inhibition was at first classified as external and only in the course of further investigation was it shown by Leporsky (11) that the conditioned inhibitory agent is not a casual extraneous one with regard to the process of internal inhibition, but, on the contrary, completely identical with it, having the same properties. Therefore, the process of conditioned inhibition was classed as internal inhibition.

Simultaneous with the described cases of inhibition of conditioned reflexes, forming the group of internal inhibitions, were noted instances of inhibition of conditioned reflexes appearing under the influence of extraneous casual stimuli. It was observed that casual stimuli, such as a sudden noise, the appearance of persons who have never taken part in the experiments, etc., arising before the action of the conditioned stimulus and during it, affect the size of the conditioned reflexes, either delaying and decreasing the secretion of saliva, or completely abolishing it [Boldirev (5), Vasiliev (20),

Zeliony (23), Mishtovt (12), and others]. But as soon as this casual stimulus begins to coincide repeatedly with the action of the conditioned stimulus, it gradually loses its inhibitory effect, ceasing to evoke in the animals the orienting reflex; therefore this kind of inhibitory agent is termed as extinguishing inhibitory stimulus. This includes the so-called *simple inhibition* of conditioned reflexes when some modification of the properties of the conditioned stimulus is followed by modifications of the conditioned reflex. Thus, if a dog eats cheese or sugar, the reflex to the bread and meat powder is inhibited, feeding of bread and meat powder inhibits the reflex to acid, etc. [Egorov (6), Perelzweig (16), Bilina (4)].

The primary classification of the processes of inhibition according to whether or not they set in under the influence of an additional agent or without it was subject to many changes, as mentioned before; at present all cases of inhibition are divided into two groups: on the one hand, *external inhibition,* always arising in the central nervous system when another point of excitation appears outside the conditioned reflex center; on the other hand, *internal inhibition,* including extinction of conditioned reflexes, retardation, conditioned inhibition, differentiation, and, finally, sleep. We must explain, however, that this division of inhibition into external and internal is not based upon the difference of the processes taking place in either of the cases, but on the difference of the causes evoking the appearance of inhibition.

All processes of inhibition, however, have another peculiarity, which has attracted the attention of investigators, the so-called *successive inhibition* carefully worked out by Vasiliev (20), Mishtovt (12), Nikolaev (14), Leporsky (11), Nikiforovsky (13), Beliakov (3), Krasnogorsky (9), and others. Successive inhibition means that any of the inhibitory agents causes inhibition not only at the moment when it is applied, but it continues to affect the subsequent positive reflexes and may do so for a more or less considerable time. This phenomenon gave rise to the supposition that the absence of visible effect when applying inhibitory agents does not result from the absence of activity in the central nervous system, but it depends upon a special process of inhibition developing in time and entirely differing from the process of excitation.

Nikolaev (14), when investigating the problems connected with conditioned inhibition, obtained the following facts, proving that in the case of inhibition of conditioned reflexes there are special processes

delaying the effect of the stimulus. Thus Nikolaev, having a conditioned reflex to light and a conditioned inhibitory combination from light plus tone, added a new stimulus to this conditioned inhibitory combination—a metronome—and reinforced this combination of three stimuli. The following observations were made: At first the metronome acquired an inhibitory effect and only after some time did it become a positive stimulus. This was explained by Nikolaev in the following way: In the first stage the metronome acquired from the conditioned inhibitory combination with which it coincided an inhibitory effect. Thus the question arose as to whether a condition caused by inhibition is a condition which is similar to that of excitation, as being able to combine and connect itself with the stimuli coinciding with it in time. In other words, can any extraneous agent, repeatedly coinciding with the process of inhibition in the central nervous system, acquire inhibitory qualities, as a stimulus repeatedly coinciding with the state of excitation acquires itself the power to stimulate. Thus the question was put and investigated by G. W. Volborth (21).

In Volborth's work the process of inhibition was brought about in two ways: In a number of experiments the process of inhibition was caused by extinction of the natural conditioned reflex to meat powder, in the other case by strongly elaborated conditioned inhibitory agents. The extinction of the natural conditioned reflex to the sight and smell of meat powder was brought about in the usual way. One of the usually applied stimuli, metronome, tone, etc., was used as an additional stimulus, which had been repeatedly applied before alone in the course of experimentation until it ceased to evoke the orienting reaction and became extinguished as an external inhibitory agent, that is, it did not inhibit positive stimuli or disinhibit the inhibitory agents. When the additional stimulus became indifferent, it was added to the extinguished stimulation to meat powder. After a number of coincidences of this indifferent stimulus with the process of inhibition caused by extinction, its effect was tried on the unextinguished stimulation. It was found that the formerly indifferent stimulus after some coincidences with the process of inhibition acquired itself inhibitory qualities (negative conditioned reflex). This acquired inhibitory effect is extended not only to those reflexes, from the extinction of which the stimulus received the inhibitory power, but also to other conditioned reflexes. The indifferent agent, coinciding with the state of conditioned inhibi-

tion, also acquired inhibitory properties (as shown by the experiments of Volborth), though not to the same degree as when coinciding with the process of inhibition caused by extinction. Thus it was proved that indifferent stimuli may acquire inhibitory properties when operating while the processes of inhibition caused by extinction and conditioned inhibition are going on in the central nervous system; that is, it was found possible by repeated coincidence of an indifferent agent with the process of internal inhibition in the central nervous system to form a conditioned inhibitory stimulus similarly as when by repeated coincidence of a formerly indifferent agent with the process of excitation of any activity we can form a conditioned positive stimulus.

It was interesting to observe how far this possibility occurs in the case of coincidence of an extraneous agent with other forms of internal inhibition (as was shown in the above-mentioned experiments with extinction and conditioned inhibition, there is a difference).

This problem was begun at the instigation of Volborth, and to fulfill the desire of Professor Pavlov to see Volborth's data elaborated. As was mentioned before, in Volborth's experiments the isolated application of the extraneous agent was repeated until it became indifferent, losing its effect as an inhibitory external agent; however, the subsequent years showed that extraneous stimuli repeated without further affecting the animal themselves cause the development of the process of inhibition in the cortex. Therefore Pavlov, examining the data of Volborth in Chapter VI of his *Lectures,* makes the following inference:

> "A point to which further reference will be made should be mentioned here, namely, that every extraneous stimulus which falls upon the hemispheres and remains without any further consequence to the animal, if repeated causes the spontaneous development of a cortical inhibition. Therefore, in the type of experiments just described, it is always necessary to ascertain the extent to which the acquisition of inhibitory properties by hitherto neutral stimuli depends on their simultaneous application with inhibitory stimuli, and to what extent these inhibitory properties have developed independently."

My task was to investigate, first, whether or not the extraneous agent repeatedly coinciding in time with the process of differentiated inhibition itself acquires inhibitory qualities, and, secondly, to

investigate to what extent the inhibitory effect of an extraneous agent applied alone, without coinciding with any particular stimulus, varies in its inhibitory power, and to compare the inhibitory effect of those stimuli which coincided with the state of inhibition.

Experimental Results

Preparing the Animals for Experimentation. To solve our problems we began experimentation on three dogs with parotid fistulae. Dog No. 1 was a young cur weighing about 18 kilograms. The formation of conditioned reflexes was begun in November, 1928. Reflexes were elaborated to the light of an electric lamp of 25 c.p., to a metronome at 96 beats per minute, and to an electric bell; the time of isolated action of these stimuli was 15 secs. The reflexes were quickly formed and soon attained their constant size. The differentiation of the metronome at 48 beats became zero after the fifth application of the differentiated metronome and was quite constant and complete; further, the differentiation was made more exact and brought up to 72 beats per minute. This period of preliminary work was completed early in April, 1929.

Dog No. 2 was also a young cur weighing about 25 kilograms, rather quiet on the stand. The formation of conditioned food reflexes was begun in October, 1928. Reflexes were elaborated to a metronome at 96 beats per minute, to a bell, and to the light of an electric lamp of 25 c. p. The time of the isolated action of these conditioned stimuli was 25 secs. The differentiation of the metronome at 48 beats per minute became complete from the 20th application of the differentiated metronome; it was changed to 72 beats per minute, and when this new frequency was applied the differentiation was complete from the very first and continued to remain so.

Dog No. 3 was a young setter weighing about 21 kilograms. The formation of conditioned food reflexes was begun in December, 1928, to a bell, a tone, and a mechanical skin-irritator—28 prickings in a minute on the right shoulder. The differentiated stimulus—another skin-irritator—was placed 8 cms. behind the positive mechanical skin-irritator. At the first two applications of the differentiated skin-irritator there was no flow of saliva, but from the 3rd to the 22nd there was a salivary secretion (4 to 10 divisions of the graduated tube), after which the differentiation became complete and constant.

Selecting Additional Stimuli and Testing Their Action as External Inhibitors

After the preliminary period was completed, i.e., after elaborating constant conditioned reflexes and well-established differentiations in the dogs, the question arose of selecting for each of these dogs two stimuli, which had not been applied before, of which one should coincide with the process of differentiated inhibition, the other to be applied separately without support. But, further, in the course of special experimentation, we had to try the action of those stimuli on positive conditioned reflexes by applying them simultaneously; consequently the question arose of excluding them, or at least of taking into account the action of these additional agents as external inhibitors.

In Volborth's investigation the possible action of these additional agents as external inhibitors was avoided in the following way: The additional agent was made indifferent by repeated, isolated application before it began to coincide with the process of inhibition. However, the statements of Pavlov prove that a repeated application for a certain length of time of any stimulus without subsequent support causes this stimulus to become not indifferent but to acquire inhibitory effect. Therefore, in the present investigation we decided to apply a single additional stimulus coinciding with the process of differentiated inhibition. From the first studies on conditioned reflexes, as well as from recent ones [Gantt and Koupalov (10), Rikman (18), Archangelsky (1), and others], it is known that the quantitative effect of the conditioned stimulus, as well as the power of any stimulus as an inhibitory agent under normal conditions, depends definitely upon the physiological strength of this stimulus, therefore in our investigation the question arose as to the selection of these two additional agents so as to have them approximately the same strength. To avoid excessive inhibition when testing the indicated additional agents, we selected them from the group of weak stimuli. We are able to judge as to the actual similiarity of these two additional agents by their inhibitory effect as external inhibitors, applying them simultaneously with the positive conditioned stimuli, and by the extent of their disinhibition of well-established inhibitory agents.

According to this outline, we tried the additional agents as external inhibitors. Our experiments showed that the selected additional stimuli were more or less of the same strength; they slightly

inhibited the positive conditioned reflexes in the same degree and disinhibited the inhibitory agents, so we could continue to solve our problems. For this purpose we made coincidences of the action of one of each two stimuli with the process of differentiated inhibition, applying the other separately without supporting it by feeding; between the described testing of the additional stimuli as external inhibitory agents and the beginning of our investigations there were intervals of two or three months.

With Dog No. 1 a weak tone coincided with the process of inhibition; the bubbling sound was applied alone. With Dog No. 2, on the contrary, the bubbling coincided with the process of inhibition caused by differentiation; the tone was applied separately. With Dog No. 3 the light coincided with the differentiated inhibition, bubbling was applied alone.

The experimentation is described for each dog separately.

Testing the Action of the Stimulus Which Had Coincided with the Process of Differentiated Inhibition and Controlling the Stimulus Which Had Been Applied Separately without Feeding

Dog No. 1. After testing the action of the additional stimuli as external inhibitory agents, we began to apply the coincidences of the tone with the differentiated metronome using bubbling without supporting it by the unconditioned food stimulus. When the tone coincided with the differentiated metronome (5 secs. for the tone, 20 secs. for the tone plus metronome, and 5 secs. for the metronome alone), this combination, which was not supported by the unconditioned food reflex, became zero after the third application, evoking no salivary flow; bubbling applied separately for 25 secs. without feeding caused a salivary secretion after the first four applications—8, 3, 3, 3 divisions of our graduated tube—at the fifth application is ceased to evoke a secretion. Besides the inhibitory combination of the tone with the differentiated metronome, we continued to apply the differentiated metronome alone once a day, its effect being always zero.

After seven applications of the tone with the differentiated metronome the action of the tone on the positive metronome was first made, the tone being given 5 secs. before the positive metronome and coinciding with it 20 secs., after which the isolated action of the positive metronome continued the last 5 secs.

TABLE 1

Dog No. 1

No. 89—April 17, 1929—fifth coincidence of the tone with the differentiated metronome (weight 17.7 kgm.)

Time	Interval between stimuli	No. of application of the given stimulus	Conditioned stimuli	Time of isolated action of the conditioned stimuli in secs.	Latent period in secs.	Size of the conditioned reflex†	Size of the unconditioned reflex†	Remarks
A. M.								
11:44	—	143	metronome 96*	25	5	18	72	
11:50	5	121	light	25	8	12	80	
11:54	3	109	metronome 72 (differentiation)	25	—	0	—	not reinforced
11:58	3	5	bubbling of water	25	—	0	—	not reinforced
12:04	5	106	bell	25	6	20	87	
12:07	2	5	tone + metronome 72	25	—	0	—	not reinforced
12:13	5	122	light	25	5	13	86	
12:17	3	144	metronome 96	25	4	17	91	
No. 92—April 20, 1929—testing the action of the tone on the positive metronome after seven coincidences of the tone with the differentiated metronome (weight 18 kgm.)								
P.M.								
1:13	—	127	light	25	16	8	68	
1:19	5	149	metronome 96	25	4	18	85	
1:24	4	115	metronome 72 (differentiation)	25	—	0	—	not reinforced
1:31	6	128	light	25	6	10	74	
1:35	3	150	tone + metronome 96	5+20+5	4	16	73	strong general motor reaction during the tone when adding the metronome the dog gives the food reaction
1:42	6	109	bell	25	6	11	84	
1:46	3	151	metronome 96	25	2	19	85	
1:51	4	8	bubbling of water	25	—	0	—	not reinforced

*Metronome at 96 beats per minute.

†The numbers give the quantity of saliva in divisions of the graduated glass tube; ten divisions are equal to one drop of water.

TABLE 2

Dog. No. 1

No. 97—May 27, 1929—13th coincidence of the tone with the differentiated metronome (weight 18.3 kgm.)

Time	Interval between stimuli	No. of application of the given stimulus	Conditioned stimuli	Time of isolated action of the conditioned stimuli in secs.	Latent period in secs.	Size of the conditioned reflex	Size of the unconditioned reflex	Remarks
A.M.								
11:23	—	137	light	25	8	13	87	
11:29	5	160	metronome 96	25	7	21	102	
11:33	3	124	metronome 72 (differentiation)	25	—	0	—	not reinforced
11:37	5	114	bell	25	7	26	101	
11:43	5	13	bubbling of water	25	—	0	—	not reinforced
11:46	2	13						
11:51	4	138	tone + metronome 72	25	—	0	—	not reinforced
			light	25	8	12	84	
11:57	4	161	metronome 96	25	7	20	92	
No. 98—May 28, 1929—testing the action of the tone on the positive metronome after 13 coincidences of the tone with the differentiated metronome (weight 18 kgm.)								
11:00	—	139	light	25	8	14	88	
11:05	4	162	metronome 96	25	7	24	93	
11:08	2	126	metronome 72 (differentiation	25	—	0	—	not reinforced
11:14	5	115	bell	25	7	26	97	
11:18	3	14	bubbling of water	25	—	2	—	not reinforced at
11:24	5	163	tone + metronome 96	5+20+5	13	12	—	the 6th sec.
11:27	2	140	light	25	9	8	85	food reaction not
11:32	4	164	metronome 96	25	7	20	91	reinforced

The data of this test are given in Table 1. As is seen from the protocol of Experiment No. 92, April 20, 1929, the tone, when added to the positive metronome, caused only a slight decrease of the salivary secretion to the metronome: instead of the usual size of 18-20 divisions of the graduated tube, we have here 16 divisions. But subsequent inhibition, shown at the following application of the stimulus—the bell—was very great; the decrease of the reflex to the bell amounted to about 50%.

After this trial we continued to apply coincidences of the tone with the differentiated metronome, and after six other similar coincidences we made the second test of the action of the tone on the positive metronome. As seen from Table 2, protocol No. 98, May 28, 1929, the tone, after 13 coincidences with the differentiated metronome, evoked, when added to the positive metronome, a marked decrease of the conditioned salivary flow. This time also a subsequent inhibition was observed affecting the light applied afterwards. We must note that in order to compare our data more favorably, we applied all the conditional stimuli in the same order on the day of experimentation as the day before—cf. protocol No. 97, May 27, 1929. Thus the inhibitory effect of the tone, after repeated coincidences with the process of differentiated inhibition was distinctly marked, and it became necessary to test the controlled stimulus (bubbling) applied without reinforcement. Table 3 represents the result of this trial. As seen from the table, there were no changes in the size of the conditioned reflex when testing the action of bubbling on the positive metronome, neither did we observe subsequent inhibition (protocol No. 101, June 1, 1929).

Dog No. 2. As was mentioned before, bubbling coincided with the process of differentiated inhibition (5 secs. for bubbling, 20 secs. for bubbling and the differentiated metronome, 5 secs. for the metronome alone), the tone being applied separately 25 secs. once a day without reinforcement. After 10 applications of the inhibitory combination the first trial was made of the action of bubbling on the positive metronome. Bubbling, after 10 coincidences with the differentiated metronome, caused a considerable decrease of the conditioned reflex to the positive metronome: instead of the usual 19-20 divisions, we obtained 8 divisions, the latent period being increased from 4-5 secs. to 9 secs. The tone, after 12 applications without reinforcement, inhibited the conditioned reflex to the metronome in a much smaller degree. After 19 coincidences of bubbling with

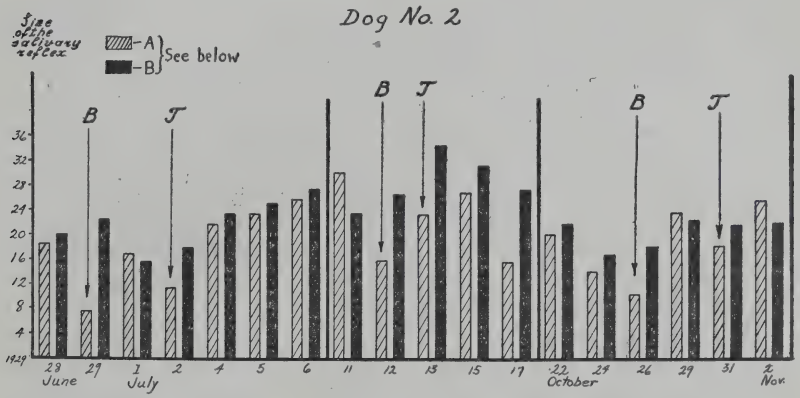
TABLE 3

Dog No. 1

No. 100—May 31, 1929—16th application of the bubbling of water ("neutral agent") (weight 18.1 kgm.)

Time	Interval between stimuli	No. of application of the given stimulus	Conditioned stimuli	Time of isolated action of the conditioned stimuli in secs.	Latent period in secs.	Size of the conditioned stimulus	Size of the unconditioned stimulus	Remarks
A.M.								
11:21	—	117	bell	25	5	23	89	
11:26	4	167	metronome 96	25	5	23	95	
11:29	2	129	metronome 72	25	16	4	—	not reinforced
11:35	5	143	light	25	4	11	82	
11:39	3	16	tone + metronome 72	25	—	0	—	not reinforced
11:44	4	168	metronome 96	25	7	20	90	
11:47	2	16	bubbling of water	25	—	2	—	not reinforced
11:53	5	144	light	25	9	10	84	
No. 101—June 1, 1929—testing the action of the bubbling ("neutral agent") on the positive metronome after 16 applications of the bubbling without coinciding with any other stimulus (weight 18.3 kgm.)								
12:00	—	169	metronome 96	25	4	19	63	
12:04	3	118	bell	25	9	21	76	
12:10	5	131	metronome 72 (differentiation)	25	—	0	—	not reinforced
12:14	3	145	light	25	8	13	78	
12:20	5	170	bubbling of water + metronome 96	5+20+5	4	22	92	stands quietly
12:26	5	146	light	25	5	15	92	
12:30	3	171	metronome 96	25	7	19	83	

the differentiated metronome and the same number of applications of the tone, we made the following tests of their action on the positive metronome. Bubbling caused a far greater decrease of the conditioned reflex to the positive metronome than it had when testing the tone. After 38 coincidences of the bubbling with the differentiated metronome and 40 applications of the tone, similar trials were again made. Now bubbling definitely inhibited the conditioned effect of the metronome, at the same time prolonging the latent period. When the tone acted, there was much less inhibition of the conditioned effect of the metronome (see Figure 1). The results obtained from all the trials on this dog of the action of bubbling (B) and the weakened tone (T) on the positive metronome are given in Figure 1, representing the sizes of the conditioned reflexes to the metronome for several days.



A—Salivary reflex to the metronome on its first application on the given day of experiment

B—Salivary reflex to the metronome on its second application on the given day of experiment

FIGURE 1

Testing the action of the bubbling of water (B), which had coincided with the differentiated metronome, on the *positive metronome,* and of the tone (T) applied once a day without coinciding with any other stimulus, "neutral agent."

First test, June 29, 1929, after 10 coincidences of the bubbling with the differentiated metronome. *Second test,* July 12, 1929, after 19 coincidences. *Third test,* October 26, 1929, after 38 coincidences.

During the test the additional agents were applied 5 secs. before the positive metronome and coincided with it for 20 secs., after which the positive metronome acted for 5 secs. alone.

All trials were made at the first application of the positive metronome on that day of experimentation represented in the Figure 1 by slightly shaded columns.

I shall not give detailed proceedings of the experiment on these two dogs, as we considered these experiments preliminary; systematic investigations were made on the third dog, which will be given below. I want only to mention here that all the data of the experiments on Dog No. 2 show us that of the two additional stimuli, the effect of which as external inhibitors was quite identical, one of these stimuli when coinciding with the process of inhibition acquired the power to inhibit in a considerable degree the positive conditioned reflexes; while the other stimulus, being applied as many times as the first one but separately and without support of the unconditioned stimulus, is far less effective in this respect.

Dog No. 3. After the results described above, we had to verify carefully and systematically the obtained data and to supplement them, viz., to test the action of both additional agents not only to the positive stimulus which had a differentiation but to other stimuli of the other analysers. For this purpose we chose as a subject of our experimentation Dog No. 3, belonging to the class of animals with a balanced nervous system. This dog always gave positive conditioned reflexes of constant strength, showing good differentiation, and during a year and a half of experimentation he never refused food in the stand, never showed motor excitement, nor sleep, nor even drowsiness. As we mentioned before, this dog had a local differentiation to the skin-irritator.

We began the coincidences of light (25 c. p.) with the differentiated mechanical skin-irritator in the same succession as with the other dogs, i.e., applying the light 5 secs. before the differentiated mechanical skin-irritator, then bringing about coincidences, but in this dog only for 15 secs. without feeding; besides this we applied the differentiated skin-irritator once a day. With the third application, the combination (differentiated mechanical skin-irritator with the light) became zero. During 15 secs. of isolated action bubbling gave 2 divisions of the graduated tube, and at the second application it ceased to evoke a secretion. After nine coincidences of light with the differentiated mechanical skin-irritator we made the first test of the action of the light on the positive skin-irritator; this trial, as well as all others, was made at the first application of the positive stimulus on a certain day of experimentation, as the first

TABLE 4

Dog No. 3

No. 89—June 20, 1929—testing the action of the light—after 9 coincidences with the differentiated mechanical skin-irritator—on the positive skin-irritator (weight 20.2 kgm.)

Time	Interval between stimuli	No. of application of the given stimulus	Conditioned stimuli	Time of isolated action of the conditioned stimuli in secs.	Latent period in secs.	Size of the conditioned reflex	Size of the unconditioned reflex	Remarks
A.M.								
10:55	—	160	bell	15	3	17	70	
11:03	6	94	tone	15	6	15	102	
11:05	4	144	light + mechanical skin-irritator A	5+15	6	8	110	
11:17	6	161	bell	15	5	13	95	
11:22	3	57	mechanical skin-irritator B (differentiation)	15	—	0	—	not reinforced
11:29	6	145	mechanical skin-irritator A	15	4	13	103	
11:35	4	8	bubbling of water	15	—	0	—	not reinforced
11:42	6	95	tone	15	7	12	98	
	No. 90—June 21, 1929—9th application of the bubbling of water ("neutral agent") (weight 20 kgm.)							
10:14	—	96	tone	15	3	18	73	
10:21	5	162	bell	15	5	16	78	
10:27	3	146	mechanical skin-irritator A	15	6	13	69	
10:21	3	9	bubbling of water	15	—	0	—	not reinforced
10:26	4	58	mechanical skin-irritator B (differentiation)	15	—	0	—	not reinforced
10:43	6	163	bell	15	9	10	95	
10:49	4	10	light + mechanical skin-irritator B	15	—	0	—	not reinforced
10:56	6	97	tone	15	10	10	86	
11:01	3	147	mechanical skin-irritator A	15	5	9	82	

TABLE 4 (*continued*)

No. 91—June 22, 1929—testing the action of the bubbling of water on the positive skin-irritator after 9 applications of the bubbling without coincidence of any other stimulus ("neutral agent") (weight 19.6 kgm.)

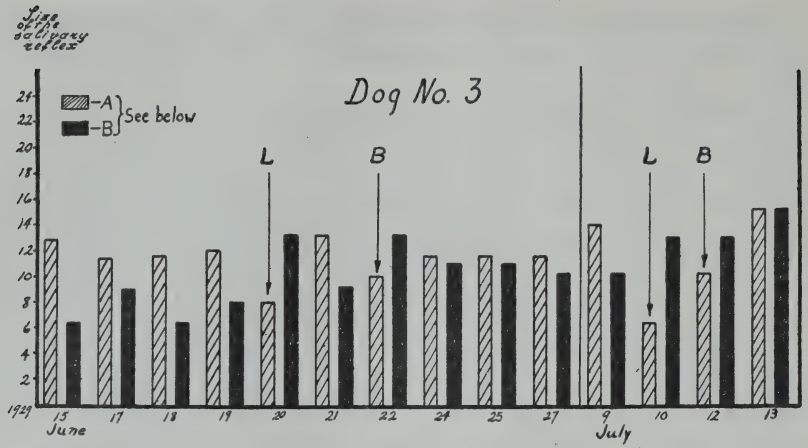
Time	Interval between stimuli	No. of application of the given stimulus	Conditioned stimuli	Time of isolated action of the conditioned stimuli in secs.	Latent period in secs.	Size of the conditioned reflex	Size of the unconditioned reflex	Remarks
A.M.								
10:15	—	164	bell	15	4	19	98	
10:22	5	98	tone	15	5	17	104	
10:27	3	148	bubbling of water + mechanical skin-irritator A	5+15	10	10	86	
10:33	4	60	mechanical skin-irritator B (differentiated)	15	12	2	—	not reinforced
10:40	6	165	bell	15	5	14	94	
10:46	4	11	light + mechanical skin-irritator B	15	8	3	—	not reinforced
10:53	6	99	tone	15	6	12	100	
10:58	3	149	mechanical skin-irritator A	15	4	13	76	
11:05	5	10	bubbling of water	15	—	0	—	not reinforced
No. 92—June 24, 1929—12th coincidence of the light with the differentiated mechanical skin-irritator and the 11th application of the bubbling of water ("neutral agent") (weight 20.4 kgm.)								
9:45	—	100	tone	15	4	18	70	
9:50	3	166	bell	15	6	15	92	
9:58	6	150	mechanical skin-irritator A	15	6	12	87	
10:00	4	12	light + mechanical skin-irritator B	15	—	0	—	not reinforced
10.00	6	167	bell	15	4	13	90	
10:00	3	151	mechanical skin-irritator A	15	4	11	78	
10:00	6	63	mechanical skin-irritator B (differentiation)	15	—	0	—	not reinforced
10:00	3	11	bubbling of water	15	8	3	—	not reinforced
10:00	5	101	tone	15	5	13	80	

applications of each positive stimulus made before using the inhibitors showed more even and constant figures than the repeated applications.

As seen from Table 4 at the first trial of the action of the light on the positive skin-irritator, there is a decrease of the conditioned reflex: instead of the usual 12 divisions at the first application, we have only 8 divisions. At the subsequent application of the bell there are no changes, the inhibitors having their usual effect (cf. protocols No. 89 and 90). Bubbling applied separately without supporting it by the unconditioned food reflex was tested also after 9 applications (see protocol No. 91, June 22, 1929, Table 4). As seen from this table, bubbling showed far less inhibitory effect on the positive skin-irritator than when the light had been applied which coincided with the differentiated stimulus. In this case the effect of the inhibitory stimuli was somewhat altered after this trial, i.e., the differentiated mechanical skin-irritation and its combination with the light; but the effect of the bubbling continued to be zero. On the day following the trial (as seen from No. 92, June 24, 1929, Table 4) all conditioned reflexes were of the usual size and only a slight disinhibition of the bubbling sound was noticed.

At the second trial of both additional agents on the positive mechanical skin-irritator, an increase of the inhibitory effect of the light was seen after 20 coincidences with the differentiated skin-irritator; at this trial the positive mechanical skin-irritator simultaneously applied with the light showed only 6 divisions instead of 13-14 divisions, though the first trial was supported by the unconditioned stimulus. Bubbling was again tested after 20 applications and showed an inhibitory effect on the positive skin-irritator; instead of 13-14 divisions, we find 10 divisions. The results of these two trials of the action of light (L) and bubbling (B) on the positive skin-irritator are given in Figure 2, which represents the effects of the positive skin-irritator for several days.

The next trial was made after 46 coincidences of the light with the differentiated mechanical skin-irritator and the same number of applications of bubbling, separately applied without supporting it by the unconditioned stimulus. The results of these experiments are given in Figure 3. As seen from Figures 2 and 3, the light caused a definite decrease of the conditioned reflex to the positive skin-irritator, when bubbling had no effect at all. The next trial after 56 coincidences of light with the differentiated mechanical skin-



A—Salivary reflex to the skin-irritator on its first application
B—Salivary reflex to the skin-irritator on its second application

FIGURE 2

Testing the action of the light (*L*), which had coincided with the differentiated mechanical skin-irritator, on the *positive mechanical skin-irritator,* and of the bubbling of water (*B*) applied once a day without coinciding with any other stimulus, "neutral agent."

First test, June 20, 1929, after 9 coincidences of the light with the differentiated skin-irritator. *Second test,* July 10, 1929, after 20 coincidences. The bubbling was tested after being applied as many times as the light.

When testing, the additional agents (*B* and *L*) were put in action 5 secs. before the positive skin-irritator and coincided with it for 15 secs. before feeding.

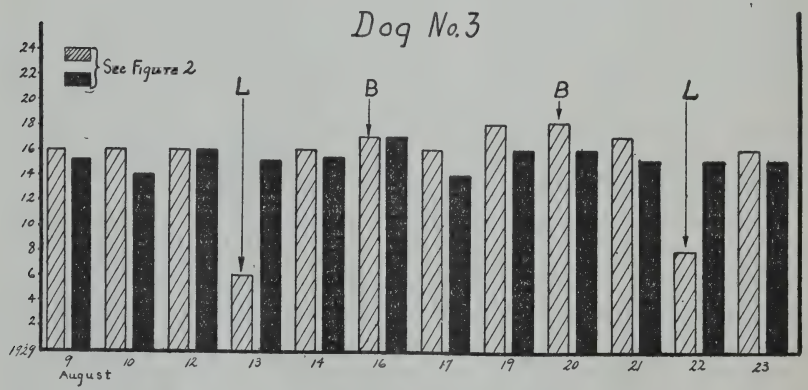


FIGURE 3

See explanation of Figure 2.

Third test, August 13, 1929, after 46 coincidences of the light with the differentiated skin-irritator.

Fourth test, August 22, 1929, after 56 coincidences.

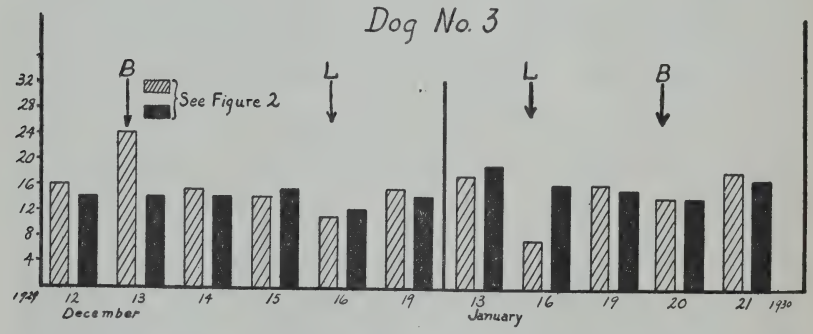
irritator gave results similar to the preceding; but this time we tested at first the action of the control agent, bubbling, on the positive skin-irritator (Figure 3). Thus if we examine the results of these four trials of the action of both additional stimuli [one of which (light) had coincided with the process of differentiated inhibition, and the other (bubbling) having been applied as many times as the light but without being supported] on the positive conditioned reflex, we observe the following: Light, when simultaneously applied with the positive stimulus, shows a marked inhibition of the conditioned reflex, causing a decrease of it and prolonging the latent period; bubbling, under the same conditions, although it had caused a slight decrease of the conditioned reflex at the first two trials (after 9 and 20 applications without feeding), not only did not decrease the conditioned reflex, but caused a slight augmentation at the subsequent trials (after 46 and 53 applications). The subsequent trials of the action of the additional stimuli on the positive mechanical skin-irritator were made under somewhat different conditions. After a six-weeks interruption following the 4th trial, we began to delay the isolated action of the tone to 1½ mins. when experimenting on Dog No. 3, the other stimuli were also delayed, being applied alone 30 secs. instead of 15 as before; the inhibitory combination and bubbling was also applied, their isolated action too being prolonged to 30 secs. The delay of the conditioned stimuli was immediately followed by a retardation of the conditioned reflexes. Without entering into details of the process of delaying the tone, I call your attention to the sizes of the conditioned reflexes to the bell and mechanical skin-irritator, and to the latent periods of these reflexes. The conditioned reflexes to both stimuli when delayed remained the same as when they were applied 15 secs.; the latent periods of these conditioned reflexes changed to a considerable extent: when applying the bell, the latent period was equal to 11-18 secs.; and when applying the skin-irritator, it was 10-17 secs. After more than two months' experimentation on Dog No. 3 the following trial was made of the action of the additional stimuli on the positive skin-irritator; at these trials the additional stimuli were applied, as formerly, 5 secs. before the positive skin-irritator coinciding with it for 25 secs., after which the skin-irritator continued to act 5 secs. longer before reinforcement.

The action of bubbling was tested first after 107 applications. It caused a considerable increase of the conditioned reflex to the posi-

tive skin-irritator and a marked reduction of the latent period, the other reflexes were not affected on that day or the day following. The light after 112 coincidences with the differentiated mechanical skin-irritator under the same conditions (Figure 4) slightly decreased the conditioned reflex to the skin-irritator. The next trials after 125 coincidences gave the same results, as shown in Figure 4.

We must now describe the results of testing the action of these additional agents on the conditioned reflexes from the other analyser. These trials were made on the bell and on the tone, the time of isolated action being 15 secs. The results of the trial of the action of light after 27 coincidences with the differentiated skin-irritator on the bell are given in Table 5, the trial of the action of bubbling after the same number of applications without feeding is represented in Table 6. At both these trials when testing the additional stimuli the bell was not supported by the unconditioned food stimulus. As seen from these tables the conditioned reflex to the bell, when the light was applied simultaneously, decreased considerably; the bell gave a much smaller figure than usual—18 instead of 22 divisions.

Bubbling, under the same conditions, produced a much smaller inhibitory effect. At the same trials all inhibitory stimuli kept their full



A—Salivary reflex to the bell (or tone) on its first application
B—Salivary reflex to the bell (or tone) on its second application

FIGURE 4

See explanation of Figure 2.

Fifth test, December 16, 1929, after 109 coincidences of light with the differentiated mechanical skin-irritator.

Sixth test, January 16, 1930, after 125 coincidences. When testing, the additional agents (*L* and *B*) were started 5 secs. before the positive mechanical skin-irritator and coincided with it 25 secs., after which the positive skin-irritator acted for 5 secs. longer before feeding.

TABLE 5

DOG No. 3

No. 106—July 18, 1929—27th coincidence of the light with the differentiated mechanical skin-irritator (weight 21.6 kgm.)

Time	Interval between stimuli	No. of application of the given stimulus	Conditioned stimuli	Time of isolated action of the conditioned stimuli in secs.	Latent period in secs.	Size of the conditioned reflex	Size of the unconditioned reflex	Remarks
A.M.								
9.26	—	129	tone	15	4	20	84	
9:35	7	195	bell	15	3	23	102	
9:40	3	25	bubbling of water	15	—	0	—	not reinforced
9:43	2	89	mechanical skin-irritator B (differentiation)	15	—	0	—	not reinforced
			mechanical skin-irritator A	15	6	13	105	
9.52	8	178	tone	15	5	19	93	
9.55	2	130	light + mechanical skin-irritator B	15	—	0	—	not reinforced
10:02	5	27	bell	15	7	22	110	
10:09	6	196	mechanical skin-irritator A	15	8	11	96	
10:14	3	179						

No. 107—July 19, 1929—testing the action of the light—after 27 coincidences with the differentiated skin-irritator—on the bell (weight 21.6 kgm.)

8:59	—	180	mechanical skin-irritator A	15	11	13	91	
9:05	4	131	tone	15	3	23	92	
9:13	6	197	light + bell	5+15	8	13	—	not reinforced
9:19	4	181	mechanical skin-irritator A	15	6	9	96	
9:25	4	26	bubbling of water	15	—	0	—	not reinforced
9:29	3	28	light + mechanical skin-irritator B	15	—	0	—	not reinforced
9:26	6	132	tone	15	7	18	88	
9:42	4	198	bell	15	5	17	84	
9:50	6	92	mechanical skin-irritator B	15	—	0	—	not reinforced

TABLE 6

DOG No. 3

No. 109—July 22, 1929—testing the action of the bubbling ("neutral agent")—after 27 applications—on the bell (weight 21.6 kgm.)

Time	Interval between stimuli	No. of application of the given stimulus	Conditioned stimuli	Time of isolated action of the conditioned stimuli in secs.	Latent period in secs.	Size of the conditioned reflex	Size of the unconditioned reflex	Remarks
A.M.								
9:27	—	184	mechanical skin-irritator A	15	4	14	75	
9:35	6	135	tone	15	4	22	103	
9:41	4	201	bubbling of water + bell	5+15	7	18	—	not reinforced
9:49	6	185	mechanical skin-irritator A	15	5	12	92	
9:54	3	136	tone	15	5	20	97	
10:01	5	30	light + mechanical skin-irritator B	15	—	0	—	not reinforced
10:05	3	28	bubbling of water	15	—	0	—	not reinforced
10:13	7	202	bell	15	5	16	102	
10:19	4	95	mechanical skin-irritator B (differentiation)	15	—	0	—	not reinforced
			No. 110—July 23, 1929—the day after the testing—(weight 21.2 kgm.)					
8:57	—	137	tone	15	5	22	68	
9:03	4	203	bell	15	6	23	98	
9:11	6	186	mechanical skin-irritator A	15	4	13	97	
9:17	4	31	light + mechanical skin-irritator B	15	—	0	—	not reinforced
9:25	7	204	bell	15	6	21	94	
9:30	3	187	mechanical skin-irritator A	15	6	13	90	
9:27	5	97	mechanical skin-irritator B	15	—	0	—	not reinforced
9:41	3	29	bubbling of water	15	—	0	—	not reinforced
9:48	6	138	tone	15	6	13	95	

TABLE 7

Dog No. 3

No. 112—July 25, 1929—testing the action of the light—after 33 coincidences with the differentiated mechanical skin-irritator—on the tone (weight 21.1 kgm.)

Time	Interval between stimuli	No. of application of the given stimulus	Conditioned stimuli	Time of isolated action of the conditioned stimuli in secs.	Latent period in secs.	Size of the conditioned reflex	Size of the unconditioned reflex	Remarks
A.M.								
8:36	—	190	mechanical skin-irritator A	15	5	13	78	
8:42	4	207	bell	15	5	24	97	
8:50	6	141	light + tone	5+15	8	10	103	
8:58	6	208	bell	15	6	18	92	
9:04	4	33	light + mechanical skin-irritator B	15	—	0	—	not reinforced
9:11	6	191	mechanical skin-irritator A	15	6	12	91	
9:16	3	142	tone	15	7	17	94	
9.23	5	31	bubbling of water	15	—	0	—	not reinforced
9:27	3	101	mechanical skin-irritator B	15	—	0	—	not reinforced

TABLE 8

Dog No. 3

No. 115—July 30, 1929—testing the action of the bubbling ("neutral agent")—after 33 applications without coincidence with any other stimulus—on the tone (weight 21.4 kgm.)

Time	Interval between stimuli	No. of application of the given stimuli	Conditioned stimuli	Time of isolated action of the conditioned stimuli in secs.	Latent period in secs.	Size of the conditioned reflex	Size of the unconditioned reflex	Remarks
A.M.								
9:07	—	213	bell	15	5	23	76	
9:13	4	196	mechanical skin-irritator A	15	4	15	89	
9:21	6	147	bubbling of water + tone	5+15	6	17	106	
9:29	6	214	bell	15	5	22	94	
9:35	4	36	light + mechanical skin-irritator B	15	—	0	—	not reinforced
9:42	6	197	mechanical skin-irritator A	15	6	12	98	
9:47	3	148	tone	15	6	15	90	
9:54	5	107	mechanical skin-irritator B	15	—	0	—	not reinforced
9:57	2	34	bubbling of water	15	—	0	—	not reinforced
			No. 116—July 31, 1929—the day after the testing (weight 22 kgm.)					
9:13	—	149	tone	15	4	23	78	
9:21	6	215	bell	15	4	25	103	
9:26	3	198	mechanical skin-irritator A	15	5	13	90	
9:34	6	216	bell	15	4	24	102	
9:40	4	108	mechanical skin-irritator B	15	—	0	—	not reinforced
9:45	4	35	bubbling of water	15	—	0	—	not reinforced
9:52	6	150	tone	15	6	19	100	
9:58	4	37	light + mechanical skin-irritator B	15	—	0	—	not reinforced
10:05	6	199	mechanical skin-irritator A	15	5	14	95	

effect. When testing the action of the additional agents on the tone similar results were obtained as with the bell (represented in Tables 7 and 8 and in Figure 5). As seen from Table 7, the light added to the tone caused a considerable decrease of the size of the conditioned reflex to the tone: instead of the usual size, 20-24 divisions of the graduated glass tube, there were only 10 divisions at this trial. When testing the control agent, bubbling, which did not coincide with any inhibitory stimulus and was applied alone without supporting it by the unconditioned food stimulus, the conditioned reflex to the tone was only a little smaller than usual, instead of 20-24 divisions it was equal to 17 divisions in this case (Table 8). Thus we see that a stimulus, after repeated coincidences with the process of internal inhibition caused by differentiation, acquires the power to inhibit conditioned reflexes, being simultaneously applied with the positive conditioned stimulus; this inhibition occurs when a weak conditioned stimulus (the mechanical skin-irritator) is applied as well as with strong ones such as the bell and the tone.

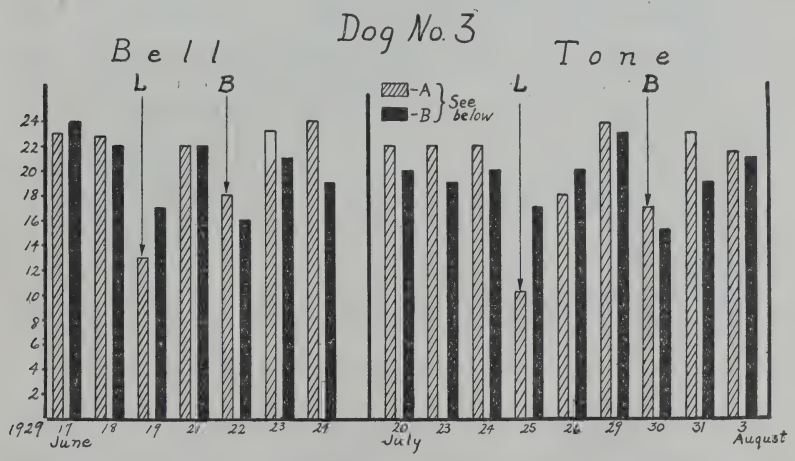


FIGURE 5

Testing the action of the light (*L*), which had coincided with the differentiated mechanical skin-irritator, and of the bubbling of water (*B*) applied once a day without coinciding with any other stimulus, on the response to the *bell* and to the *tone*.

Testing the *bell*, July 19, 1929, after 27 coincidences of the light with the differentiated skin-irritator.

Testing the *tone*, July 25, 1929, after 32 coincidences.

Conclusions

From all these observations we may draw the following conclusions:

1. The possibility of forming inhibitory agents of stimuli having no connection with any reactions of the dog, by making their action coincide with the process of inhibition caused by extinction of conditioned reflexes and by conditioned inhibition, as indicated by Volborth, applies also to other forms of internal inhibition (differentiation).

2. Stimuli which have repeatedly coincided with the process of inhibition caused by differentiation show an inhibitory effect on the positive conditioned reflexes applied simultaneously with the conditioned stimuli.

3. This inhibitory effect manifests itself in regard to the conditioned stimulus with the differentiation from which the additional agent coincided, as well as the conditioned reflexes to other stimuli.

4. A stimulus having no special relation to any activity of the animal after repeated isolated applications, without coinciding with any particular activity of the animal, produces an inhibitory effect on positive conditioned reflexes when applied simultaneously; however, the inhibitory action of this stimulus is far less effective and less constant than that of the tone, which had coincided with the process of inhibition.

5. As the number of repetitions of the isolated stimulus increases its inhibitory effect diminishes, an effect which we do not observe with the stimulus that coincides with the process of inhibition caused by differentiation.

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LA FORMATION DE RÉFLEXES NÉGATIVEMENT CONDITIONNÉS PAR UNE COÏNCIDENCE DE TEMPS AVEC LE PROCESSUS DE L'INHIBITION DIFFÉRENTIELLE

(Résumé)

Les travaux de Pavloff ont montré que lorsqu'un agent externe agit en même temps que l'excitation propre, cet agent devient capable par lui-même de produire l'excitation de l'écorce cérébrale. Récemment Volborth a prouvé que ceci également vrai pour l'inhibition—c.a.d. n'importe quel action ayant lieu simultanément avec l'inhibition du système nerveux central acquiert par lui-même a la propriété de provoquer l'état d'inhibition.

Volborth a conduit ses experiences sur l'inhibition qui a été induite par l'extinction des réflexes conditionnels naturels ou bien par l'inhibition conditionnelle très prononcée. Lindberg dans ce travail démontre que ceci est également vrai pour les actions qui agissent parallèlement avec l'inhibition, produite par cet agent externe lorsqu'il est employé seul, subit des variations.

Ses expériences sur trois chiens l'ont amené aux conclusions suivantes. L'inhibition a une influence sur les réflexes conditionnels provoqués par d'autres stimulants aussi bien que sur ler réflexes conditionnés provoqués par les stimulants differenciés avec lesquels l'inhibition agissait parallélement. Un excitant neutre après avoir été appliqué plusieurs fois, provoque un état d'inhibition dans les réflexes conditionnés positifs; cependent, l'inhibition est moins forte et moins constante que l'inhibition de l'excitant qui a accompagné les processus de l'inhibition. LINDBERG

DIE FORMATION VON NEGATIVEN BEDINGTEN REFLEXEN WENN DER ÄUSSERE STIMULUS MIT DEM DIFFERENTIELLEN HEMMUNGSPROZESS GLEICHZEITIG IST

(Referät)

Pawlow hat gezeigt, dass, wenn ein äusserer Stimulus mit dem Erregungsprozess zeitlich zusammenfällt, dieser Stimulus seinerseits die Eigenschaft erwirbt, dem Erregungszustand im Cortex hervorzurufen. Vor kurzem hat Volborth bewiesen dass dies auch für die Inhibition gilt, d.h., wenn ein äusserer Stimulus gleichzeitig mit einem Hemmungsprozess im Zentralnervensystem auftritt, bekommt er die Fähigkeit von sich aus den Hemmungszustand herbeizuführen.

Volborth führte diese Hemmungsexperimente mit Hilfe von natürlichen oder stark entwickelten bedingten Reflexen durch. Lindberg zeigt nun in dieser Veröffentlichung, dass auch Stimuli, die zeitlich mit differenzierten Hemmungen zusammenfallen, diese Eigenschaft haben, und er beschreibt, zu welchen Grade der hemmende Einfluss eines äusseren Stimulus variiert.

Seine Experimente an drei Hunden führen zu folgenden Ergebenissen: die Hemmungswirkung erstreckt sich nicht nur auf den Differentialstimulus, sondern auch auf andere Stimuli. Zweitens wird hervorgehoben, dass ein neutraler Stimulus (d.h., ein Stimulus, der mit keiner bestimmten Tätigkeit des Tiers zusammengefallen ist) nach mehreren Wiederholungen, einen hemmendem Einwirkung auf gleichzeitige positive bedingte Reflexe hat, dass aber dieser Hemmungseffekt weniger kräftig und weniger konstant ist, als die Wirkung des Stimulus der primär die Hemmung hervorgerufen hatte. LINDBERG

BEHAVIOR CONSTANCY*

From the Psychological Laboratory of the University of California

WILLIS D. ELLIS AND JAMES A. HAMILTON

I

In proposing in two previous articles (3, 4) the term "Behavior Constancy" for the purpose of characterizing certain phenomena observed in the post-operative behavior of brain-injured rats, we were constrained to restrict theoretical considerations in order that the experimental evidence be presented in the clearest light possible. A word at this time concerning the origin and meaning of the concept may not, however, be inappropriate.

We shall refer, on the one hand, to Lashley's work with rats, on the other, to Gelb's observations of war patients suffering from permanent brain injuries. Lashley (5, pp. 43, 45) found that operated rats solved a simple discrimination box problem more readily than normal rats. The differences were not great, but their significance is considerably enhanced when one remembers that, in the ten major problems set, the results of nine were decidedly in favor of normal animals. One not unnaturally wishes to know something more regarding the nature of these various problems. There were three kinds: mazes, brightness discriminations, and relearnings. Both upon their first experience with mazes and upon the occasion of relearning the same, normal rats gave better results than the operated. In relearning the brightness discrimination, normal animals were again superior (though but slightly) to the operated. *Original* learning in the discrimination box, however, indicates a certain *advantage* in favor of brain injury. This, in fact, would seem to be the most fruitful attitude to adopt when considering the problems themselves—in what sort of task is brain injury an *advantage?*

Before pursuing this question further, let us turn to the findings of Gelb and Goldstein (2). The sort of thing we have in mind does not come out in published articles, wherein the author is reporting actual experiments, so readily as from consideration and observa-

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tion of the patients in their everyday lives. If, for example, one inquires of Rtg. what he did yesterday, one is told of this and that with a readiness which belies any suspicion of the patient's normality. Among other things Rtg. will mention having gone for a walk. Asked where he went, one is told street for street and corner for corner the precise route taken. Still there is nothing unusual in this. If, however, the same questions are asked upon the following day, the answers will be exactly what they were before. This has continued for fifteen years. Narrowing our interest down to the "walk" we find here, as Gelb suggests, precisely the opposite of Goethe's:

"Ich ging im Walde so für mich hin
und nichts zu suchen, das war mein Sinn."

Rtg. does not go for a walk—he marches. The route is as fixed as if he were "set" by some mechanism to travel so many paces, turn right, go such and such distance, turn left, and so forth.

Take another case. Attendants at the *Hirnverletzenheim* secured work in a bottling factory for the patient Pf. His duties were to wrap and pack bottles. After some weeks the factory reported that never before had they had a more satisfactory employee for this task. Indifferent—indeed, in this case oblivious to all surrounding occurrences—Pf. packed bottles with the accuracy, precision, and rhythm of a machine.

It is needless to multiply examples, but one more general instance may be cited. The *Lazarett* occupied by these patients during the war was unfortunately a wooden structure, woefully inadequate and lacking in many conveniences. In 1920 the people of Frankfurt provided a special hospital for this particular group of war patients. Of recent construction, comfortable, and surrounded by gardens, the new home afforded advantages one might well suppose the patients would greatly appreciate. There occurred instead a near rebellion. Accustomed to their original quarters, practiced in overcoming its inadequacies, fixed in the habits they had adopted during the early weeks of physical convalescence, they considered the interruption calamitous.

II

Returning to the Lashley problems and the question of advantage raised in that connection, let us consider what *kind* of behavior would probably prove most appropriate for maze problems and what kind we would expect to be most adequate in a discrimination box. If it were

Rtg. or Pf. or some other war patient instead of rats, there is reason to believe that the kind of condition in which the patient would be most at home would be one requiring a minimum of self-initiated modification in behavior. A task requiring mechanical precision and rhythmic behavior together with a corresponding indifference to possible distractions in order to yield the best results will probably be as well or better accomplished by man or animal provided with befitting characteristics than by normal creatures subject to normal flexibilities of behavior. The bottle factory found that normal workmen, whatever the manifoldly complex causes may have been, were less satisfactory than Pf. Monotonous and uninteresting, the task of packing bottles left normal workers with a fairly high percentage of unused time in which to think about other things, worry about family relationships, or in some other way decrease, however slightly, their efficiency in trying to do what a machine would doubtless have done better. Pf., on the contrary, again whatever the precise causes may have been, showed by objective comparison that he was better fitted for this kind of work than normal men were.

In the case of operated and normal rats confronted with a discrimination box problem it appears that the box must have called for the *kind* of behavior operated animals were able to engage in *more* than the kind which normal rats exhibit. One of the most common observations of all rat experimenters is that the animals display a sometimes downright annoying proclivity to "snoop around" instead of running along the path to food. Even hungry, well-trained animals will occasionally, for no particular reason, delay their reaction in favor of sniffing at cracks or rearing up against the wall of a maze or discrimination box in which they are placed. Though a detailed account of the respective behaviors is not given by Lashley, we nevertheless read that in general "the whole picture [of post-operative behavior] is one of a deterioration rather than of a loss which is specific for the mazes" (5, p. 139).

Comparing the considerations already suggested, it would seem that in the case of Lashley's discrimination box the advantage enjoyed by operated rats as against normal ones lay chiefly in the fact that the former went about the business of learning to choose the lighted opening and avoid the darker one, whereas, *in addition to this*, normal rats were encumbered with the further "responsibility," let us call it, of gratifying their curiosity, nosing in corners and, in general, delaying their (from the experimenter's point of view) proper occu-

pation of learning to ignore the darker side. In Lashley's maze problems the probably greater urge to gratify curiosity did not interrupt the behavior of normal rats to an extent sufficient to constitute a disadvantage. Enhanced sensitivity to environment (inquisitiveness, e.g.) is probably an advantage in a maze whereas in a discrimination box it may prove an actual deterrent. The blinds in Lashley's mazes were equal in length and we may suppose that all rats were equally hungry. Had the hunger been less, it is possible that normal rats would have been more delayed proportional to their degree of hunger than operated rats proportional to *theirs*.[1] An experiment bearing on this point has been reported by Tolman, Honzik, and Robinson (7), who found that in a maze with long and short blinds, in which hungry and relatively non-hungry rats were used, the long blinds were eliminated last by non-hungry animals and first by hungry ones. According to the viewpoint here suggested, it would appear that this difference arose from the fact that *non-hungry* rats, released from the drive which more or less "compelled" the hungry ones to reach food as soon as possible, were in a position to indulge *another* "drive," namely, that of curiosity satisfaction.

Bringing together these several suggestions and experimental results, answers to the following questions were sought (3, 4): (1) Can Lashley's findings be reproduced, and, if so, will the resulting behavior resemble the *kind* of behavior exhibited by the patients studied by Gelb? (2) Is there anything so typical in the behavior of organisms with brain injury that a common terminology applicable to all such cases can be set forth? Finally, (3) what seems to be the *characteristic* thing about such behavior?

To investigate these problems a series of experiments was undertaken. Unlike the problems studied by Lashley, but resembling more nearly the matters observed by Gelb, the problems did not deal with learning to *do* but rather with learning to *undo* a certain activity. Just as no one had prescribed the exact route Rtg. should follow when going for a "walk," so in this case as many details were left to the animal himself as possible. However, just as Rtg. of his own accord did adopt a fixed route for his march through the city streets, so also it was found that the rats, operated and normal alike, soon developed a regular routine for getting to food. Again, unlike Lashley, no possibility of error was provided. The apparatus in one

[1]Lashley suggests (p. 133) that the felt hunger of operated rats may have been even greater than that of the normal animals.

case consisted of a box resembling a discrimination box except that there was but one opening from the main chamber to food.[2] In the other case a box was provided in which the rat was allowed to pull in string to obtain food. Here also he either pulled or not, but nothing about his behavior in the pulling-box could be constituted an "error."

Without reviewing the results in detail, it is sufficient to report that with *operated* rats the habit was found to be more fixed, more "crystallized" into a self-subsistent behavior-unit, apparently more determined from within itself in the way it went off and hence less pliably susceptible to changes in the familiar conditions than was the case with *normal* rats. The original sequence of behavior acts was appropriate for the original set-up—the animals had not only adapted themselves to this set-up but had been permitted thoroughly to familiarize themselves with such procedure. The original sequence of behavior acts was *not* appropriate, however, after the experimenter had made certain changes in the apparatus.[3] Despite this fact it was found that operated rats continued in their familiar and erstwhile appropriate behavior longer than normal rats did. In other words, the behavior-unit of normal animals was more readily disrupted than was that of operated animals. It is from considering certain implications of the term "behavior-unit" that one is led to an understanding of both these findings and those reported by Lashley and Gelb.

As Tolman (6, Chap. V, ¶ 2) has very rightly urged, "behavior cannot go off *in vacuo*." For me to pick up a pencil there must be pencils in the vicinity. As Tolman puts this, "A rat cannot 'run down an alley' without an actual floor to push his feet against, actual walls to steer between, actual free space ahead to catapult into." Manifestly *behavior* is best defined as the activity of a living organism that either is or is not functioning in-and-of an environmental matrix. If the behavior succeeds, we may say that the relationship "in-and-of" regarding the organism's activity and the environment thereof *does*

[2]Hence the term *discrimination* is, of course, inadequate. It is used, however, merely to designate that a type of box was employed which more or less resembled the customary device with the exception that no discrimination between exits was required.

[3]These changes consisted in the first case in removing food from the food box; in the second set-up (pulling-box) the rats were fed to satiation so that performing the behavior leading to, securing, and eating food was thereupon rendered "inappropriate."

obtain; if it fails, this relationship is lacking. For the sake of simplicity let us drop the clumsier terminology and say merely that the organism either is or is not behaving "*in*" the given environment. This may be summarized and illustrated as follows:

Situation I is that which prevails when the rat is running smoothly through a maze. The animal's activity we may designate in this case as Behavior I. Now comes the experimenter and changes the apparatus so that Behavior I is necessarily incommensurate with the new set-up.[4] The animal encounters this revised apparatus and Behavior I either does or does not disappear. The fate of Behavior I must tell the story of what the animal's functional rôle in Situation I has been. Three possibilities may be distinguished:

a. If Behavior I is supplanted by a "Behavior II" which is appropriate to Situation II, we may say that the animal has solved the problem presented by Situation II, or, in other words, that Situation II now prevails rather than some other. "Appropriate," of course, is to be defined in terms of the goal sought and in so far as the experimenter can say what that goal for the animal is, to that extent can he judge regarding the appropriateness of this or that behavior (cf. 1).

b. If Behavior I disrupts but is not followed by a behavior congruent with Situation II, the animal has not solved the problem. Disruption of Behavior I which is not followed by an appropriate Behavior II gives us only negative results. If the experimenter has introduced changes such that Behavior I is physically impossible, the bare disruption of Behavior I merely corroborates his intentions to eliminate this behavior but gives no necessary evidence to substantiate the inference that the animal has or has not become aware of the conditions now prevailing. But *some* other behavior necessarily supplants Behavior I. However, so far as the *experimenter's* notion of what the problem is is concerned, any inadequate or inappropriate behavior indicates that that problem has not been solved.

c. If Behavior I *continues* we may record not only "no solution" but also evidence that for the animal Situation I still prevails. The reason may be that the experimenter erred in thinking the changes he had introduced were sufficient to destroy Situation I. Or it may be that his physical alterations are effective for certain subjects only. In any case, we are dealing at this juncture with the phenomenon of *behavior constancy*. It should be apparent at once, however, that no

[4]He knows *how* to change it only after having seen what Behavior I is.

evaluation is implied by this term. All that it designates is a means of classifying the degree to which organisms are sensitive to environmental changes. A difference in such sensitivity was found, for instance, by Hamilton and Ellis as regards operated and normal rats. Both groups displayed a certain degree of behavior constancy but the "Behavior I" of operated rats continued longer than did that of normal animals.

In none of these factors alone, however—neither Situations I and II nor Behaviors I and II—can the whole story be found. A description of "Situation I," for example, which omitted reference to the animal's behavior would not *be* a description of Situation I, for an essential and integral part of the total set-up is the animal himself. Indeed, precisely that which makes the alleys and blinds of this maze a *maze* is that an organism is placed therein and attempts to find his way from one end to the other. Similarly with a description of "Behavior I," as mentioned above, unless supports for the behavior be present it will not and *cannot* occur. The total complex, the entire *Gestalt* either is or is not available for description and inquiry. This, then, is what was meant above by the term *behavior-unit*. A behavior-unit is a total of behavior acts the internal organization of which is such that differentiation is relatively slight. A behavior-unit is one molar event (6, Chap I, ¶ 3 *et seq.*) running its course in such fashion that as one "thing" it is essentially sensitive throughout. An interruption of the *Gesamtverlauf* which occurs early, late, or midway will cause disruption not alone locally but of the behavior-unit as a whole. But this molar unit of behavior, far from being independent of the environmental milieu is, rather, a functional aspect thereof, and in this fact lies the possibility of objective study of behavior. Specification of the characteristics and relationships of environmental matrices in-and-of which an organism performs as functional part constitutes a description that is both psychological and objective.

IV

In concluding, let us consider again the questions proposed above. The animals studied displayed a degree of behavior inflexibility which, when compared with that of *the same* animal while normal, appears to resemble the *kind* of behavior characteristic of Gelb's war patients. Moreover, since a behavior-unit would seem to be a *behavior-unit* regardless of the specific acts involved, one is probably

justified in employing the same terminology whether describing the behavior of animals or men. The characteristic thing about the behavior-units of brain-injured organisms is, so far as present observations permit us to speak, a decided crystallization, fixation, or inflexibility of the unit such that it tends to maintain itself unchanged despite environmental changes over against which a modified form of behavior would be more appropriate. Normal organisms, whether men or animals, supply the *norm* in terms of which it can be asserted that brain injury enhances behavior constancy and thus diminishes the possibility of insight into unfamiliar environmental relationships.

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LA CONSTANCE DE COMPORTEMENT

(Résumé)

On cite des ressemblances, entre quelques observations de Gelb et de Goldstein sur les êtres humains avec lésions cérébrales, et quelques-unes des conclusions de Lashley sur les rats avec lésions cérébrales. On considère celles-ci en relation avec des expériences déjà rapportées par les auteurs, où des rats normaux et des rats opérés ont été entraînés dans certaines situations définies. Quand les animaux ont appris à fond ces situations, on a changé celles-ci de sorte que le comportement qui y correspondait auparavant n'y correspondait plus. On a étudié la manière de laquelle le comportement a été modifié pour être adapté à la situation nouvelle, et l'on a constaté que le comportement des rats opérés reste toujours le même malgré les modifications.

On a comparé ces résultats avec ceux d'autres expériences sur les hommes et les animaux et l'on suggère que cette cristallisation, fixation ou résistance aux changement du milieu est une caractéristique de tous les organismes avec lésions cérébrales. On suggère le terme "Constance de Comportement" pour décrire le phénomène. On appuie sur le fait que la "Constance de Comportement" sera un avantage dans la résolution de certains types de problèmes.

ELLIS ET HAMILTON

VERHALTUNGSKONSTANZ

(Referat)

Es wirt auf Änlichkeiten zwischen einerseits einigen Beobachtungen von Gelb und Goldstein an Menschen mit Gehirnverletzungen und andererseits einigen Befunden von Lashley an gehirnverletzten Ratten hingewiesen. Diese Änlichkeiten werden in Bezug auf Experimente berücksichtigt, worüber die Verfasser schon früher Bericht erstattet haben, und worin normale und operierte Ratten zur Reagierung auf besondere bestimmte Situationen dressiert worden sind. Nachdem diese Situationen den Tieren vollständig vertraut geworden waren, wurden erstere so geändert, dass ein Verhalten, die ihnen vorher angepasst gewesen war, es nun nicht länger war. Man untersuchte die Weise, auf die das Verhalten modifiziert wurde, um sie der neuen Situation anzupassen, und man fand, dass das Verhalten operierter Ratten trotz Modifizierungen der Situationen unverändert blieb.

Diese Befunde wurden mit denen aus anderen Arbeiten an Menschen und Tieren verglichen, und es wurde angedeutet, dass eine gewisse *Konstanz,* dieses Verhaltens gegen Änderungen der Umgebung ein Karakteristikum aller gehirnverletzter Organismen darstellt. Der Ausdruck "Verhaltungskonstanz" (behavior constancy) wird als den Befund beschreibend vorgeschlagen. Es wird betont, dass "Verhaltungskonstanz" bei der Lösung gewisser Arten von Aufgaben von Nutzen sein wird.

ELLIS UND HAMILTON

SEX DIFFERENCES IN EMOTIONAL RESPONSES*

EDWIN G. FLEMMING

Although all previous studies that disclosed sex differences also showed that even where there is a sex difference there is more or less overlapping, so that differences within the sex group are greater than differences between the sex groups, the problem of sex differences is not yet a completely solved problem. Popular opinion, as well as the private opinions of psychologists, still insists that there are some differences, although scientific investigations have not yet revealed what everybody feels is there.

Former studies have been largely investigations of differences in intelligence and mental characteristics, and of differences in certain non-intellectual traits. The purpose of this study is to investigate emotional differences.

Besides the general question—are there any emotional differences between the sexes—we shall try to answer more specific questions, some of which are as follows:

1. Are women's emotional responses more intense than those of men?
2. Do women show greater variability in a given series of emotional responses than do men?
3. Do women show greater changes in their emotional responses from one time to another than do men?
4. Do women show more reversed responses; that is, do they change more often from pleasant to unpleasant, from blameworthy to praiseworthy, from fearful to joyful responses or vice versa, than do men?
5. In the realm of sex itself are there any differences between the emotional responses of men and of women?

The subjects used were two groups of men and two groups of women taking a compulsory course in psychology. The first group of men consisted of 89, the second of 75. There were 125 women in the first group and 42 in the second. They were mostly of sophomore rank, with a sprinkling of upper classmen, but no freshmen.

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The fact that the course was compulsory assures the probability of a fairly random sample of college men and women.

The instrument used to measure their emotional responses was the Flemming test for measuring emotional responses, which is a revision of the Pressey XO test for investigating the emotions. All the words in Parts 1, 3, and 4 of the Pressey test are used. They are, however, arranged in columns, and after each word are the numbers from minus five through zero to plus five.

The instructions for Part A of the revised test are as follows:

> Opposite each of the following words draw a circle around the number that indicates the degree of pleasant or unpleasant meaning that the word has for you. If the word has a decidedly unpleasant meaning, draw a circle around the minus 5 at the extreme left; if it has a decidedly pleasant meaning, draw a circle around the 5 at the extreme right; if it is neither pleasant nor unpleasant in meaning to you, draw a circle around the 0. If it is only relatively pleasant or unpleasant, draw a circle around the number which most adequately represents the relative degree of pleasantness or unpleasantness. The minus sign signifies the unpleasant side of the scale; no sign represents the pleasant side of the scale.
>
> DO NOT STUDY OVER YOUR RESPONSES; IMMEDIATE JUDGMENTS ARE DESIRED. ENCIRCLE ONLY *ONE* NUMBER FOR EACH WORD, BUT BE SURE TO INDICATE YOUR RESPONSE TO *EVERY* WORD.

Directions for Parts B and C of the test are similar to those for Part A, except that Part B calls for blameworthy and praiseworthy responses, and in Part C the subject is asked to indicate whether the words indicate something worrisome, fearful, or dreadful, on the one hand, or joyful, on the other.

The test was given twice to the first group of men and to the first group of women. Thus the extent of the change upon the second test could be figured, and the number of times that the subjects changed their responses from the plus to the minus side or from the minus to the plus side of the scale on each word. Other than to get a measure of such emotional consistency and to figure reliability, the second groups of men and women were not used except to check any apparently significant results obtained with the first groups.

By correlating the average rating on each of the words for the first and second administrations of the test we find that for measuring

TABLE 1

SHOWING THE RELIABILITY COEFFICIENTS FOR THE AVERAGE RESPONSES FOR BOTH MEN AND WOMEN ON EACH PART OF THE TEST

	Men	Women
Part A	.9879	.9911
Part B	.9831	.9866
Part C	.9741	.9841

TABLE 2

SHOWING THE CORRELATIONS BETWEEN THE AVERAGE RATINGS FOR EACH WORD OF TWO SEPARATE GROUPS OF MEN AND WOMEN

	Men	Women
Part A	.9807	.9794
Part B	.9696	.9744
Part C	.9699	.9712

group emotional responses the test is highly reliable. The reliability coefficients for each part of the test for both men and women are shown in Table 1.

Not only is the test exceedingly reliable for measuring group averages when repeated, but it is equally reliable when given to a second group as shown by the correlations, in Table 2, between the averages of the first and second groups.

There can be no question, then, of the reliability of the test as a measure of group responses. The standard errors of estimate on the basis of the correlations in Tables 1 and 2 are from .3 to .5.

Since the test is so highly reliable for both men and women any statistically reliable differences found would be of much significance.

When we correlate the average ratings by the first group of men with the average ratings by the first group of women we get a coefficient on each part of the test of .97, which would seem to indicate that there are no sex differences. The standard error of estimate in each part is .5.

The average ratings, that is, the average of the averages for each part of the test, together with the differences, the sigmas of the differences, and differences divided by the sigmas of the differences are shown in Table 3.

TABLE 3

SHOWING THE AVERAGE RATINGS OF MEN AND WOMEN, THE DIFFERENCES, AND THE RELIABILITY OF THE DIFFERENCES

Part	Averages		Difference	Sigma of difference	Difference Sig. diff.
	Men	Women			
A	—1.00	—1.07	.07	.32	.002
B	—1.91	—1.91	.00	.26	.000
C	.89	.97	.08	.23	.003

TABLE 4

SHOWING THE AVERAGE VARIABILITY OF MEN AND WOMEN, THE DIFFERENCES, AND THE RELIABILITY OF THE DIFFERENCES

Part	Averages		Difference	Sigma of difference	Difference Sig. diff.
	Men	Women			
A	2.03	1.85	.18	.055	3.24
B	1.86	1.78	.08	.054	1.53
C	2.06	1.98	.08	.046	1.77

There is no reliable difference, then, between the responses of the men and the responses of the women on the Flemming test of emotional responses.

Differences in variability between men and women are sometimes remarked, although scientific psychological investigations have not shown any of the supposed differences to be reliable. The variability of the responses of men and women on each word of the test used was compared, with the results indicated in Table 4.

Here we have a statistically reliable difference between men and women with respect to variability in their pleasant-unpleasant responses. Contrary to the general notion, men seem to be more variable than women in the things they regard as pleasant and unpleasant.

If this difference is really a *sex* difference and not merely a reliable difference between the two particular groups used in the study, the difference should show again when we compare the variability of the first group of women with the second group of men. When such a comparison is made we find that the difference is only .03 and the difference divided by the sigma of the difference .61. So we cannot say that men, as men, are more variable than women, as women, in their pleasant and unpleasant responses.

In the comparison of average ratings on the test the intensity of the pleasant responses, of the blameworthy responses, and of the joyful responses was subtracted from the intensity of the corresponding responses on the negative side of the scale. A more accurate measure of intensity of emotional response is to add the intensities of the minus and plus sides of the scale, since a pleasant response, for example, of four is just as intense as an unpleasant response of four. By considering the scale as continuous we do not get a real measure of intensity, but rather a measure of the response on the test, which indicates whether the average response is pleasant or unpleasant, toward the negative side of the scale or toward the positive side.

When we compare the intensity of responses, regardless of whether the response is on one side of the scale or the other, we get results indicated in Table 5.

TABLE 5

SHOWING THE INTENSITY OF THE RESPONSES OF MEN AND WOMEN, THE DIFFERENCES, AND THE RELIABILITY OF THE DIFFERENCES

Part	Averages		Difference	Sigma of difference	Difference / Sig. diff.
	Men	Women			
A	2.09	2.25	.16	.158	1.01
B	2.26	2.34	.08	.149	.54
C	1.56	1.61	.05	.111	.45

Although in each case the average response of the women is a little more intense than the average response of the men, the differences are not statistically reliable. But we must not confuse intensity, as measured by this test, and expressiveness, in which the author found in another investigation a realiable difference between men and women.

When the ratings on the second test were compared it was observed that some of the subjects changed their responses in some instances from positive to negative, or negative to positive. Now, why would a subject change his mind about what he considered pleasant? Why should a thing or an activity be pleasant at one time and unpleasant at another? Perhaps some of the words indicate things that may be pleasant in one setting and unpleasant in another. But that still does not answer the question as to why the one connotation should appear at one time and another at a later period. Per-

haps, then, they represent mere errors in perception, or carelessness, or slips. In that case they would be of significance to the Freudian, for slips have their fundamental cause as have all our actions. If such slips represent inhibitions, a study of the changes from plus to minus or minus to plus would throw some light upon the relative amount of inhibitions of men and women.

TABLE 6

SHOWING THE AVERAGE PERCENTAGE OF MEN AND WOMEN WHO CHANGE THEIR RESPONSES FROM PLUS TO MINUS OR MINUS TO PLUS ON THE VARIOUS WORDS OF THE TEST WHEN THE TEST IS REPEATED

Part	Averages		Difference	Sigma of difference	Difference Sig. diff.
	Men	Women			
A	4.6	4.5	.1	.46	.22
B	3.8	3.0	.8	.32	2.35
C	5.4	4.8	.6	.70	.86

An examination of Table 6 shows that men on every part of the test make a larger percentage of changes from plus to minus or minus to plus. If it is true that such changes represent slips, or inhibitions that are showing themselves in spite of the subject, then the Freudian insistence that women have more inhibitions than men is in error.

On blameworthy-praiseworthy responses the difference is almost reliable; indeed, the chances are 99 in 100 that the difference is reliable. The blameworthy-praiseworthy responses may be considered to represent an ethical standard. In that case it appears that the ethical standard of the women is more definitely fixed than that of men. However, such a possible difference in fixity of ethical judgments may be influenced by maturity, since women of college age are generally considered to be somewhat more mature than men of the same corresponding age.

If the changes from one side of the scale to the other are considered to indicate mere changes of mind due to the external and internal chances of the moment, then the popular notion that women change their minds more frequently than men is in error. This error is more clearly brought out by comparing the total amount of change in response when the test is repeated, which comparison appears in Table 7.

TABLE 7

SHOWING THE AVERAGE AMOUNT OF CHANGE ON EACH WORD OF MEN AND WOMEN WHEN THE TEST IS REPEATED, THE DIFFERENCE, AND THE RELIABILITY OF THE DIFFERENCE

Part	Average		Difference	Sigma of Difference	Difference Sig. diff.
	Men	Women			
A	1.40	1.25	.15	.037	4.15
B	1.23	1.21	.02	.033	.60
C	1.39	1.32	.07	.036	1.96

On all parts of the test the men show greater changes in response than do the women; while on the pleasant-unpleasant section of the test the difference is statistically reliable. The women, evidently, know what they like and what they do not like rather more definitely than do the men; and they know more definitely how much they like or dislike a thing.

If the lack of consistency, as shown by the extent of the change, and by the changes from one side of the scale to the other, means emotional conflict and possible emotional instability, then men tend to show more conflict than women. But such a tendency to change may not mean at all conflict and instability; but rather it may mean a greater adaptability to the passing exigencies of the moment; in which case men would appear to be more adaptable and adjustable.

But whatever meaning we assign to the changes in response, the general notion that women change their minds more often and to a greater extent than do men is untenable. An explanation of the apparent change of mind of women is that men, who, if anything, actually change their minds more than women, insist that the women should agree with them. Through superior physical strength men have come to overawe women; but the women, still holding to their opinions, instead of stating their feelings in contradiction to the wishes of the men and thus raising an issue—from which experience has probably taught them that they will emerge the vanquished—have learned to use tact, to withold overt acquiescence with the men, and to remain silent. Men, through the urgency of their desire, assume that the women by their silence have agreed with their own changing feelings, and when they learn that the women have remained steadfast, the defense mechanism operates and the men loudly assert that women are constantly changing their minds so that it is impossible to tell what they want.

Women, on the other hand, say that you cannot trust women; not because they change their minds, but because their silence in critical situations in the face of a physically superior force does not reveal their real attitude. You cannot believe what women say, or appear to say; and you cannot predict what they will do, since the force of the moment may compel them to act contrary to their convictions.

Taking up our last question, whether there are any sex differences in the realm of sex itself, we can compare the responses on the sex-colored words only. If there is a significant difference in the percentage of changes from plus to minus or minus to plus responses on sex-colored words it might throw light upon the theory that women are more inhibited sexually than are men.

There is a total of 57 sex-colored words in the three parts of the test; however, some would not include a few of the words and would include others as being sex-colored. But even though there might be some difference of opinion about which words were sex-colored, on the majority of them there would be no disagreement; so, on the basis of the 57 selected, any significant tendencies in sex differences ought to show themselves if they exist at all.

A study of these sex-colored words shows that, on the average, 5.1% of the men change their responses on these words from one side of the scale to the other, while 4.1% of the women make similar changes. The difference is 1%; and the difference divided by the sigma of the difference is 1.34. The chances are, then, about 90 in 100 that the difference is reliable. While the figures do not meet the formal requirements for statistical reliability, the difference is reliable enough to suggest that the group of men studied are not as certain about their attitude toward sex as the women are.

Up to this point we have considered the differences in the average responses for each part of the test. Let us now examine the average responses for each word; perhaps there may be some sex differences on particulars that are not revealed by comparisons of totals.

We do, in fact, find a number of statistically reliable differences between the average responses of men and women of the first group to specific words. They are shown in Table 8.

Before we comment upon these differences or draw any conclusions, we need to be as sure as we can that they represent genuine sex differences and not merely differences between the men and women of our first group under the specific conditions determined at the time the test was given.

TABLE 8

WORDS ON WHICH THERE IS A RELIABLE DIFFERENCE BETWEEN THE AVERAGE RESPONSES OF THE FIRST GROUP OF MEN AND THE FIRST GROUP OF WOMEN

	Averages			Sigma of	Difference
	Men	Women	Difference	difference	Sig. diff.
		Part A			
Sex	1.96	— .07	2.03	.350	5.80
Naked	—1.27	—2.47	1.20	.341	3.52
Breast	1.44	— .61	2.05	.292	7.02
Worm	—1.89	—2.93	1.04	.280	3.71
City	1.46	2.57	1.11	.314	3.54
Laughter	3.48	4.50	1.05	.275	3.82
Undress	.56	— .76	1.32	.286	4.62
Journey	3.01	4.06	1.05	.268	3.92
Drunk	—2.13	—3.83	1.70	.394	4.31
Cheat	—3.79	—4.42	.63	.197	3.20
Leg	1.18	— .04	1.22	.264	4.62
Baby	2.36	3.48	1.12	.306	3.66
Yellow	— .31	1.19	1.50	.296	5.07
		Part B			
Childish	—2.91	—2.19	.72	.234	3.08
Sporty	.17	1.32	1.15	.364	3.16
Gang	—1.97	—1.04	.93	.237	3.92
Prizefight	.31	—1.70	2.01	.358	5.61
Shabby	—1.99	—1.29	.70	.228	3.07
Pick-up	—1.41	—2.46	1.05	.320	3.28
Mistress	— .33	—1.61	1.28	.417	3.07
Trust	1.39	2.86	1.47	.370	3.97
		Part C			
Women	2.31	.69	1.62	.269	6.02
Sex	2.04	.28	1.76	.289	6.09
Gun	.07	—1.40	1.47	.361	4.07
Medicine	— .37	—1.45	1.08	.336	3.21

A check on these results can be made, since the test was given to the first group of men and the first group of women twice, with an interval of about two months. It was also given to a second group of men and to a second group of women the following year.

First, we would want to eliminate those words on which there is a reliable difference between the first and second responses of men, and on which there is a reliable difference between the first and second responses of the women. None of the words in Table 8 shows a reliable difference between the first and second ratings of the men; but *laughter, cheat,* and *trust* show reliable differences between the first and second responses of the first group of women—so they must

come out of the list that shows a reliable difference between men and women.

Comparing the results from the first test of the first group of women with those from the second group of men, we find that there is no reliable difference between the ratings on *childish, sporty, gang,* and *shabby;* they, therefore, must be eliminated as words upon which there is a genuine sex difference.

Between the first group of women on the first test and the first group of men on the second test there is no reliable difference on *sex* and *undress;* so we shall eliminate them from further consideration.

Reliable differences found on the first test for the first group of women should hold up on the second test for the women if they are real sex differences. But after comparing the ratings of the first group of women on the second test with the ratings of the first group of men on their first test, we must eliminate *naked, worm, city, journey, baby, mistress,* and *medicine,* since the differences in the average responses do not continue to be statistically reliable.

By further comparison between the second responses of the first group of women and the responses of the second group of men, we must eliminate *yellow* and *women.*

No further eliminations are called for from among the words that are left when comparing the second responses of the first group of women with the second responses of the first group of men.

But a comparison of the ratings of the second group of women with the ratings of the first group of men on the first test requires the elimination of *drunk, prizefight,* and *gun.* Still further comparisons between the second group of women, and the second group of men, and the second test of the first group of men, suggest no further eliminations, except that between the second group of women and the first group of men, second test, the difference on *sex* in Part C divided by the sigma of the difference is 2.97. This is so very near the statistical requirement, the word has withstood all the other comparisons, and the difference is in every case more than one full point, that it seems permissible to include it among the words on which there is a definitely reliable sex difference between the responses of the men and those of the women.

After eliminating all the words from the original list contained in Table 8 about which there could be any serious question, there are four words remaining in response to which there seem to be statistically reliable sex differences in emotion. They are shown in Table

9 together with the differences and the reliability of the differences found by comparing the responses of various groups.

In addition to these differences there is also a reliable difference of .93 in the amount of change in the ratings on the pleasant-unpleasant nature of the word *drunk*. The difference divided by the sigma of the difference is 3.60. Women, evidently, are more certain than the men about the pleasantness or unpleasantness of being drunk.

But notice in Table 9 that all four words are sex words. We may say that there are definitely reliable sex differences in the field of sex itself. Men find *breast* and *leg* pleasant; women find them unpleasant. No doubt this difference is due to experience; but there is also no doubt from this study that the emotional reactions of unmarried college men and women are reliably different.

Pick-up both men and women consider blameworthy, but women consider it much more so than do men. This difference, again, is undoubtedly due to experience and teaching; but that it also represents a basic difference in behavior patterns of men and women is also probable.

To the word *sex* the women on Part C respond with relative indifference, the average rating being around .25; but the men find it joyful, their ratings being between 1.40 and 2.04.

Taking those instances in which the differences are the smallest, we find as usual a certain amount of overlapping. Twenty-two per cent of the men find *breast* more unpleasant than do 50% of the women; 34% of the men find *leg* more unpleasant than do 50% of the women; 35% of the men find *pick-up* more blameworthy than do 50% of the women; and 39% of the men find *sex* more fearful or less joyful than do 50% of the women. So, as has been found in the study of other characteristics, emotional differences are greater within the sex group than between the sex groups.

But the many sex differences found in the many studies cannot be argued away by emphasizing the overlapping. If even slight, though statistically reliable, differences were of no real significance, all men and women would be alike or nearly alike, could be treated in practically the same manner, and their behavior quite definitely predicted within certain limits of variability. But such is not the case. Women are not merely men wearing skirts; and a woman in shorts is not thereby metamorphosed into a man.

No doubt the emphasis upon the amount of overlapping gained the ascendancy, with the result that sex differences were considered neg-

TABLE 9

SHOWING THE WORDS ON WHICH THERE ARE RELIABLE SEX DIFFERENCES IN EMOTIONAL RESPONSE BETWEEN VARIOUS GROUPS OF MEN AND WOMEN

Groups compared	Pleasant-unpleasant- Breast Diff.	Pleasant-unpleasant- Breast D / S.D.	Leg Diff.	Leg D / S.D.	Blameworthy-praiseworthy Pick-up Diff.	Blameworthy-praiseworthy Pick-up D / S.D.	Fearful-joyful Sex Diff.	Fearful-joyful Sex D / S.D.
Women, Group 1, Test 1, compared with:								
Men, Group 1, Test 1	2.05	7.02	1.22	4.62	1.05	3.28	1.76	6.09
Men, Group 1, Test 2	1.57	5.83	.81	3.16	.91	3.00	1.12	3.89
Men, Group 2	2.13	7.29	1.72	5.97	1.65	4.64	1.39	4.63
Women, Group 1, Test 2, compared with:								
Men, Group 1, Test 1	2.12	7.60	1.20	4.72	1.22	3.96	1.82	6.09
Men, Group 1, Test 2	1.64	6.45	.79	3.22	1.08	3.74	1.18	3.96
Men, Group 2	2.20	7.91	1.70	6.11	1.82	5.33	1.45	4.69
Women, Group 2, compared with:								
Men, Group 1, Test 1	2.01	5.19	1.47	4.41	1.78	4.44	1.83	4.54
Men, Group 1, Test 2	1.53	4.14	1.06	3.11	1.64	4.23	1.19	2.97
Men, Group 2	2.09	5.41	1.97	5.39	2.38	5.56	1.46	3.56

ligible and unimportant, because the interest in sex differences was coincident with the rise and success of the feminist movement. The tendency would naturally be for even scientists to fall in line with the prevailing mode of thought if it were at all possible, and consequently similarities received the emphasis to the possible detriment of the truth.

Let us suppose that on one hundred characteristics there were no average differences whatever between men and women, only the normal variability. In that case the chances would be even that a man picked at random would be different from a woman picked at random upon any one of the given characteristics.

But if on each of the hundred traits the differences were statistically reliable that the men rated higher than the women, that men possessed more of the trait than women, then the chances would not be even but would be 70 or 80 or 90 or more in 100, depending upon the difference and its reliability, that a man picked at random would possess more of any one of these given traits than a woman likewise picked at random. So where there are statistically reliable sex differences they are genuine, definite, and significant differences; and the chances are more than even that any one man and any one woman will differ in those characteristics.

But of equal importance with the differences on any one trait is the possibility of ramifications in behavior patterns of the difference in basic characteristics. Consider the differences that are bound to ensue upon a difference in interest and pleasure derived from *leg* and *breast,* or the behavior patterns that would develop from a genuine difference in ability to name colors.

While it is important and valuable to analyze behavior into its component parts, into specific traits or characteristics, nevertheless, the combinations and permutations of sex differences must not be neglected—at any rate, not in our discussions, interpretations, and theories, however difficult they may be to study in experiment.

That there are sex differences has been often shown; that too much emphasis has heretofore been put upon similarities is probable; and that combinations and permutations of differences have been neglected is all too evident.

Conclusions

1. The Flemming revision of the Pressey XO test for investigating the emotions is highly reliable as a group measure. The reliability

coefficients for each of the three parts of the test for both men and women are .97 or better.

2. Taking the test as a whole, there appear to be no sex differences. The correlations between the average ratings of men and the average ratings of women on each part of the test are .97 or more. There is likewise no reliable difference between the average of the averages on any part of the test.

3. There is no consistently reliable difference between men and women in the variability of their responses on the Flemming test of emotional responses.

4. There is no reliable difference between the intensity of the emotional responses of men and women.

5. A smaller percentage of women than of men change their emotional responses from pleasant to unpleasant, from blameworthy to praiseworthy, and from fearful to joyful, or vice versa. However, the differences are not statistically reliable; but in the case of blameworthy-praiseworthy responses the chances are 90 in 100 that a smaller percentage of women than of men change their responses from one to the other.

6. Women do not change their minds to a greater extent than do men. In fact, the situation is, if anything, just the reverse; and in pleasant-unpleasant responses the difference is statistically reliable.

7. Five per cent of the men, on the average, change their responses on sex words from one side of the scale to the other, whereas only 4% of the women, on the average, so change. The chances are 90 in 100 that the difference is reliable.

8. There is a reliable difference between the amount of change in the ratings of men and women on the word *drunk*. Women, apparently, are more certain about the pleasant or unpleasant nature of being drunk.

9. There are definitely reliable differences between men and women in their emotional reactions to *breast, leg, pick-up,* and *sex.* Men find *breast* and *leg* pleasant; women find them unpleasant. Both men and women find *pick-up* blameworthy; but women consider it more so than do men. Women are relatively indifferent to *sex;* while men react to it with joy rather than fear.

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LES DIFFÉRENCES DE SEXE DANS LES RÉPONSES ÉMOTIVES

(Résumé)

En employant la révision Flemming du test Pressey pour étudier les émotions, et deux groupes d'hommes (89 et 75) et deux groupes de femmes (125 et 42) on a trouvé ces résultats:

1. Les femmes ne montrent pas une intensité plus grande que celle des hommes dans leurs réponses émotives dans le test, et elles ne sont pas plus variables.
2. Les hommes tendent à changer leurs réponse émotives plus fréquemment que les femmes; dans les réponses agréables-désagréables la différence est statistiquement constante.
3. Les hommes tendent à renverser leurs réponses émotives plus souvent que les femmes; dans les émotions où il s'agit de censure ou de louange il est probable dans 99 cas sur 100 que le pourcentage des femmes qui changent leurs réponses de l'une à l'autre à la répétition du test sera plus petit que celui des hommes.
4. Il est probable dans 90 cas sur 100 que les hommes renverseront leurs réponses émotives aux mots de sexe plus fréquemment que les femmes.
5. Les femmes sont plus certaines sur la nature agréable ou désagréable du mot "ivre." La différence est statistiquement constante.
6. Il existe des différences définiment et statistiquement constantes entre les hommes et les femmes dans leurs réactions émotives aux mots "sein," "jambe," "personne accostée," et "sexe." Les hommes trouvent agréables "sein" et "jambe;" les femmes les trouvent désagréables. Les hommes et les femmes trouvent "personne accostée" digne de censure; mais les femmes trouvent ce mot plus digne de censure que les hommes. Les femmes sont relativement indifférentes au sexe; tandis que les hommes y réagissent avec joie plutôt qu'avec peur.

FLEMMING

GESCHLECHTSUNTERSCHIEDE IN BEZUG AUF GEFÜHLSREAKTIONEN

(Referat)

Unter Verwendung der Flemming'schen Revision des Pressey Test zur Untersuchung der Gemützustände an zwei Gruppen von Männern (respektiv aus 89 und 75 Vpp. bestehend) und zwei Gruppen von Frauen (respektiv aus 125 und 42 Vpp. bestehend) kam man zu folgenden Befunden: (1) Frauen sind in ihren Gefühlsreaktionen auf den Test nicht heftiger, als Männer, und sind auch nicht variabler. (2) Männer neigen dazu, ihre Gefühlsreaktionen mehr zu ändern, als Frauen; in Bezug auf genehm-unangenehme Reaktionen ist dieser Unterschied ein statistisch zuverlässiger. (3) Männer neigen dazu, ihre Gefühlsreaktionen öfter als Frauen umzustellen [reverse]. Bei Gemütsbewegungen, in denen Tadel oder Lob in Anspruch genommen wird, wird in 99 Fällen aus 100 ein kleinerer Prozentsatz der Frauen als der Männer bei Wiederholung des Tests die Reaktion von der einen zu der anderen Seite wechseln. (4) In 99 aus 100 Fällen werden Männer ihre Reaktionen auf Wörter, die sich auf sexuale Sachen beziehen, häufiger umstellen, als Frauen. (5) Frauen sind über die angenehme oder unangenehme Beschaffenheit des Wortes 'betrunken' sicherer,

als Männer. Dieser Unterschied ist statistisch zuverlässig. (6) Es bestehen bestimmte und statistisch zuverlässige Unterschiede zwischen Männern und Frauen in Bezug auf ihre Gefühlsreaktionen auf die Wörter 'Brust,' 'Schenkel,' 'Strassenbekanntschaft' [pick-up], und 'Geschlecht.' Männer empfinden 'Brust' und 'Schenkel' als angenehm; Frauen empfinden sie als unangenehm. Sowohl Männer wie Frauen empfinden 'Strassenbekanntschaft' als tadelnswert, aber Frauen empfinden den Tadel stärker, als es Männer tun. Frauen sind geschlechtlichen Dingen [sex] gegenüber relativ gleichgültig, während Männer darauf eher mit Freude als mit Angst reagieren.

FLEMMING

CERTAIN QUANTITATIVE ASPECTS OF THE FOOD-BEHAVIOR OF CATS*[1]

From the Harvard Psychological Laboratory

W. A. Bousfield

Since behavior cannot be studied adequately by the consideration of temporally discrete reflexes, it is desirable to establish quantitative indices of an orderly progression of responses wherever this is possible. Food-behavior is well adapted to this kind of study since continuous records may be made of the amount of food eaten by an animal during a given time interval. That graphical records of feeding possess a high degree of regularity, when obtained under controlled conditions, has been well demonstrated by the experiments of Skinner (2, 3). The importance of this work, however, is not from the psychological point of view merely the possibility of describing a certain type of behavior by a mathematical equation. It is, rather, that this technique may be used as a means of investigating such phenomena as drive, learning, and affectivity. Preliminary studies in this field are necessarily concerned with the standardization of procedure, and the establishment of indices capable of quantitative manipulation. This paper describes graphical representations of the food-behavior of cats, and the derivation of an exponential equation to fit the actual "eating"-curves.

The procedure was planned with the purpose of simulating as closely as possible the usual eating conditions of the animals. The food used during the experiment was a mixture of finely ground fillet of sole and enough milk to make a mixture of the consistency of a heavy soup. Before using, the food was heated to about 94° F. Except when the animals were in the experimental cage, they were given a regular diet made up of cooked oatmeal, Hamburg-steak, and milk.

The recorded apparatus consisted of a Pelouze scale with a capacity

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[1]The writer wishes to acknowledge an indebtedness to Dr. M. H. Elliott for his interest and help in the undertaking of this experiment.

of 500 gms., and a delicate lever system which transmitted the movement of the scale-pan to a light celluloid point resting against the surface of a kymograph drum. Food was poured into an aluminum dish and placed on the scale, which in turn was inserted under a circular opening in the platform on which the animals were fed. Decrease in the contents of the food-dish during eating was followed by a rise of the scale-pan and was recorded through the lever system upon the kymograph drum.

Feeding took place twice a day, at which times each animal was given approximately 125 gms. of the regular food. On the mornings of alternate days, the experimental food was used and records of the eating were obtained. In every case there was no limit to the amount of food the animals were allowed to eat, so that cessation of eating presumably meant satiation as far as the experimental food was concerned. It may be noted, however, that the animals would regularly accept beef-kidney, a more desirable food, though they would not return to the fish-and-milk mixture.

Sample eating-curves, together with the theoretical curves, are shown in Figure 1. It may be seen that these curves approach horizontal asymptotes. A threshold for cessation is implied. It was observed that a distracting noise, such as the opening of a door, would bring about complete checking of eating when the curves were becoming horizontal, but such noises caused no noticeable change in the eating when the slopes of the curves were relatively steep. A retardation of the speed of eating is regularly followed by a compensatory acceleration which returns the curve to its theoretical course. This phenomenon was noted by Skinner (3) in the eating-curves of the rats used in his experiments, and he found it to take place even when a cessation of eating was deliberately brought about by cutting off the supply of food. Results of the present experiment amply confirm this finding. The compensatory effect fails to return the actual eating-curve to its theoretical value, however, if food is withheld relatively late in the eating-period when the strength of the drive is presumably reduced.

A casual examination of the records indicates that the theoretical curve should be either an exponential or a power function; that is, the true curve should approximate some form of either $y = cx^n$ or $y = ce^{mx}$. The equation to be preferred depends obviously on which of the two gives the better fit in a large number of cases. Any curve selected for analysis should be a complete record of food-be-

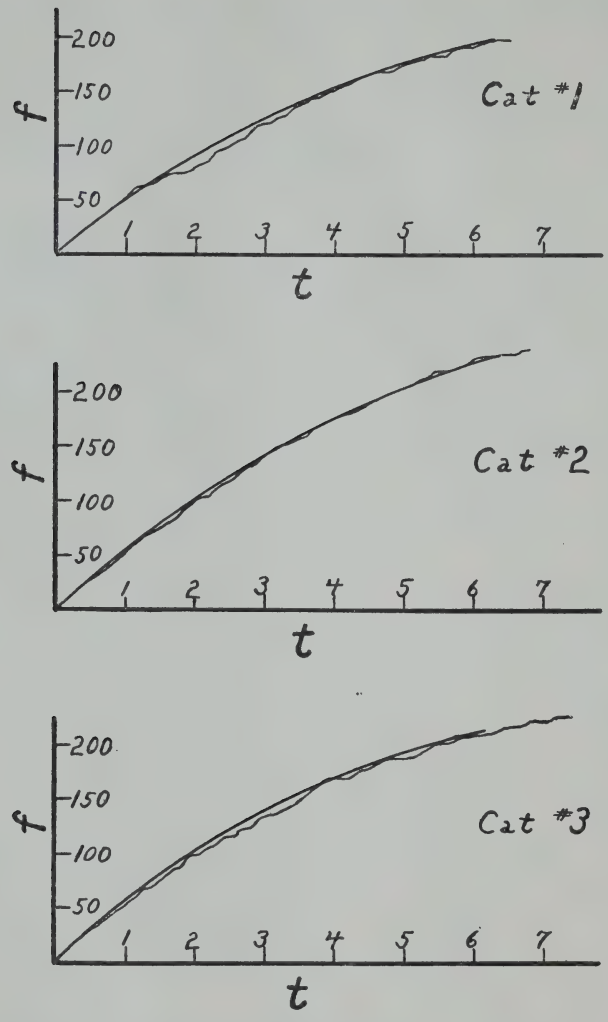

FIGURE 1

SAMPLE EATING-CURVES FOR THREE CATS WITH THEORETICAL CURVES SUPERIMPOSED

Ordinates: gms. of food eaten; abscissae: time in minutes

havior from the commencement of eating to the point of satiation. If an equation is to be chosen, it should, accordingly, give a fairly accurate representation of the complete progression of eating. The testing of the equations is fortunately a relatively simple matter.

The following procedure was used in the present experiment. A smooth curve was first drawn through the actual eating-record to include all the high points. The record was then placed on a sheet of regular or arithmetic cross-section paper having 20 squares per inch (400 per square inch). Over regular intervals on the smoothed eating-curve, points were punched with a pin so that the smoothed record could be duplicated on the arithmetic cross-section paper. For convenience, units of ten squares were chosen, and coordinates designated by these units were plotted directly on logarithmic cross-section paper.

The requirements of the power function, $y = cx^n$, are fulfilled if the values obtained from the actual record (in this case the values from the arithmetic cross-section paper) lie on a straight line when plotted on logarithmic cross-section paper. On the other hand, if the exponential function, $y = ce^{mx}$, more accurately describes the record, the points lie on a straight line when plotted on single or semi-logarithmic cross-section paper. In either case, if the plotted points form a uniform curve concave upward, a constant, b, may be subtracted from all the y's. If the curvature is downward, a constant b may be added to the y-values. A third alternative which avoids the inconvenience of dealing with negative values consists of subtracting all the y's from the constant b. Accepting the approximation to the straight line as the basis of choice, the third alternative used in connection with semi-logarithmic cross-section paper appeared to give the best results.

In Figure 2 are represented typical records of the animals in Figure 1 plotted on semi-logarithmic coordinates. All y or food-values have been subtracted from a constant in each case. The equation represented may be expressed:

$$f = c\ (1 - e^{mt}) \qquad [1]$$

The factors involved are: f = amount of food eaten; c = constant; e = base e of natural logarithms; m = constant; t = time.

An example of the derivation of the equation is as follows. An eating-curve of cat No. 3 is duplicated on arithmetic cross-section paper. The coordinates obtained are: $x = 1$, $y = 0.80$; $x = 2$, y

$= 1.55$; $x = 3$, $y = 2.25$; $x = 4$, $y = 2.85$; $x = 5$, $y = 3.35$; $x = 6$, $y = 3.70$; $x = 7$, $y = 4.00$; $x = 8$, $y = 4.30$. All of the y-values are subtracted from the constant 6 so that they become 5.20, 4.45, 3.75, 3.15, 2.65, 2.30, 2.00, and 1.70 respectively. The new ordinates together with the original x-values are then plotted on the semi-logarithmic cross-section paper, and the result is a fairly close approximation to a straight line. The constant "6" is empirically determined. When values of "5" or "7" are tried, there is less of an approach to the straight line. Values of the constants in the empirical equation may now be determined

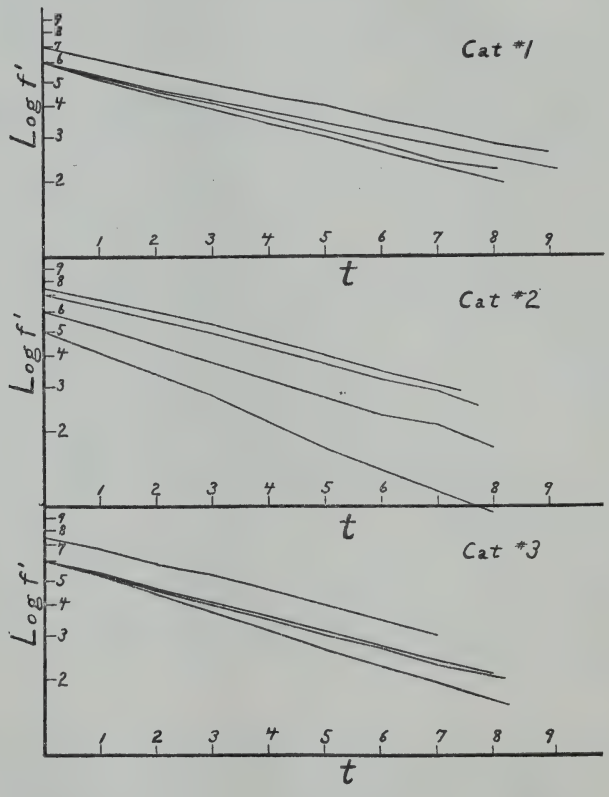

FIGURE 2

SAMPLE RECORDS OF ANIMALS IN FIGURE 1 PLOTTED ON SEMI-LOGARITHMIC COORDINATES AS DESCRIBED IN THE TEXT

directly. The value of c is represented by the empirically selected "6." The slope, m, is represented by the ratio of the log of 6 to the intercept of the plotted line on the x-axis, or 0.778 divided by 11.2, which gives 0.0695. But this slope is based on common logarithms. The value may be expressed as an exponent of base e of natural logarithms by multiplying the figure by 2.3026, which gives 0.160. The slope is negative, however, so that m must actually be —0.160. The equation may now be written,

$$f = 6\ (1 - e^{-0.16t}) \qquad [2]$$

Since it is convenient to express f in gms. of food consumed and t in minutes, equation [2] must be further modified. When the smoothed record was plotted on the arithmetic cross-section paper, one y-unit represented 50 gms. of food, and one x-unit, 0.784 min. The constant c is consequently multiplied by 50 while m is divided by 0.784. The final equation is, therefore,

$$f = 300\ (1 - e^{-0.204t}) \qquad [3]$$

The average of the c-values in the present experiment was about 316.67, while the average of the m-values was 0.1745.

The rate of eating is represented by the first derivative of equation [1],

$$\frac{df}{dt} = -cme^{mt} \qquad [4]$$

Acceleration of the eating process, or the rate at which the eating varies with time, is represented by the second derivative,

$$\frac{d^2f}{dt^2} = -cm^2e^{mt} \qquad [5]$$

The procedure outlined for the determination of the constants in equation [1] holds equally well for the derivation of the original form of the exponential function, $y = ce^{mx}$, since c is the value of y when $x=0$, and m is the slope divided by the value of the x-unit.

If the power function, $y = cx^n$, were found to hold, which would be the case if the best straight line were obtained by plotting the original values on logarithmic cross-section paper, c would represent the value of y when $x = 1$. The slope n is in this case simply measured with the aid of an auxiliary uniform scale.

In comparing the exponential function with the power function, it becomes apparent that the curves represented by these equations are

close together when the time or x-values are relatively small. There is an interweaving so that, when the first halves of the curves are considered, fairly straight lines may be obtained by transferring the values to either logarithmic or semi-logarithmic coordinates. When relatively large time-values are considered, the power equation definitely ceases to describe the food-behavior since the amount of food indicated may be far beyond the capacity of the animals. The exponential equation does not give unreasonable values because there is a relatively quick approach to horizontal asymptotes. Since there is a slight sigmoid tendency in some of the eating-curves of the cats, implying what may be termed a "warming-up" process, the power function which implies a greater initial slope further falls short of an adequate description.

The essential characteristics of the exponential and power functions are shown in Figure 3. Theoretical curves, obtained by the methods outlined, have been plotted together with the original record. It may be seen that the power curve definitely ceases to describe the food-behavior after about 6 minutes. The slightly lower initial slope of the exponential curve is a closer approximation to the original record than is the slightly steeper slope of the power curve.

The equation which Skinner chooses to describe the food-behavior of rats is of the power variety (2, p. 437), which raises the ques-

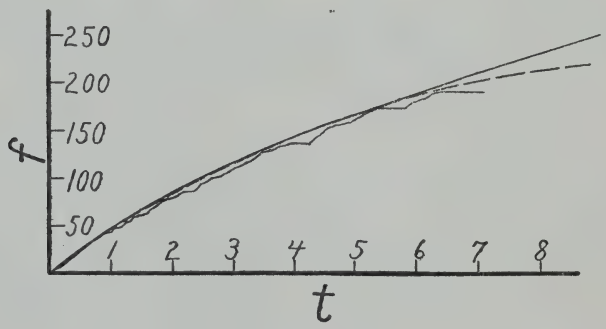

FIGURE 3

ORIGINAL "EATING" RECORD, TOGETHER WITH THEORETICAL EXPONENTIAL AND POWER CURVES

The exponential curve (plotted in the broken line) is derived from the equation, $f = c\ (1 - e^{mt})$. The power curve (plotted in the full line) is derived from the equation, $f = ct^n - b$. Ordinates: gms. of food eaten; abscissae: time in minutes.

tion of the possibility of differences in the behavior of different animals eating different foods. On the basis of an examination of Skinner's published records, it may be ventured that the exponential equation can also be applied to the rat data, and can describe the food-behavior of rats quite adequately; furthermore, the exponential function is, on the whole, superior to the power function as a descriptive formula. The main reason for this contention is, as noted above, the failure of the power function to describe food-behavior as satiation is approached. A graphical demonstration of this feature may be seen in the pronounced bending of some of the logarithmic plots of Skinner's data (2, p. 436) in the region of higher food values. In the second place, there is the possibility that satiation is not approached in certain of Skinner's records, since the amount of food eaten in the apparatus, sometimes about 5 gms., is relatively small for the daily ration of an adult rat (1, p. 150). The addition of 5 gms. of surface-dry lettuce leaf after the rats were removed from the apparatus would still make a relatively small daily consumption. It has been shown that both the exponential and power functions give close approximations to the food-behavior for the lower food values, but it is preferable that the equation should follow, as closely as possible, the actual behavior to the point of satiation for the given type of food.

The true equation for food-behavior may be more complex than the simple exponential, but the very close approximation to the theoretical curve when the total eating process is considered warrants the use of this equation. The constants are easily obtained and thus furnish the possibility of comparison on a quantitative basis.

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CERTAINS ASPECTS QUANTITATIFS DU COMPORTEMENT DES CHATS EN PRÉSENCE DE LA NOURRITURE

(Résumé)

Au moyen d'une simple technique de l'enregistrement, on peut obtenir des notations graphiques continues de la quantité de nourriture mangée par un chat pendant une durée donnée. On considère en détail le processus mathématique pour la détermination de formules exponentielles empiriques et de formules de puissance pour décrire le comportement en présence de la nourriture. Une analyse des résultats indique qu'on peut les décrire le mieux par l'équation exponentielle, $f = c(1—e^{mt})$, où f= la quantité de nourriture mangée, c = constante, e = base e des logarithmes naturels, m = constante, et t = temps. Puisque les premières courbes indiquent une diminution relativement rapide de la vitesse de manger comme on approche la satiété, la supériorité de la fonction exponentielle sur la fonction de puissance, comme employée par Skinner, se montre par le fait que la courbe exponentielle approche plus facilement d'une asymptote horizontale. La pente de la courbe de puissance reste comparativement rapide dans la région des valeurs supérieures de nourriture. Puisque certains des résultats indiquent une petite retardation initiale de l'action de manger, la courbe exponentielle est encore plus adéquate à cause de sa pente initiale moins rapide. La généralité de la fonction exponentielle est indiquée par le fait qu'on peut l'appliquer aux courbes de nourriture des rats employés dans les expériences de Skinner.

BOUSFIELD

QUANTITATIVE BETRACHTUNGEN ÜBER DIE FRESSTÄTIGKEIT BEI KATZEN

(Referat)

Mit einem einfachen Registrierungsapparat war man im Stande, fortfahrende graphische Registrierungen der Quantität des von einer Katze im Laufe eines gewissen Zeitintervals verzehrten Futters zu erhalten. Das mathematische Verfahren zur Bestimmung der empirischen Exponential- und Potenzformeln zur Beschreibung der Fresstätigkeit wird ausführlich berücksichtigt. Eine Analyse der Registrierungen weist darauf hin, dass sie am Besten mit der Exponentialgleichung $f = c\ (1—e^{mt})$ beschrieben werden kann, worin mit f die Quantität des verzehrten Futters, mit c eine konstante Grösse, mit e die Basis e der natürlichen Logarithmen, mit m eine konstante Grösse, und mit t der Zeitverbrauch angedeutet wird. Da in den ursprünglichen Kurven auf eine relativ rasche Abnahme in der Schnelligkeit des Fressens mit Herannahung der Sättigung hingewiesen wird, so ist die Exponentialfunktion der von Skinner verwendeten Potenzfunktion darin überlegen, dass die Exponentialkurve sich leichter horizontalen Asymptoten annähert. Die Schrägflache [slope] der Potenzkurve bleibt im Bereich der höheren Fresswerten relativ steil. Da, einigen der Registrierungen nach, eine geringe anfängliche Verzögerung des Fressens besteht, so erweist sich die Exponentialkurve, ihrer niedrigeren anfänglichen Schräge [slope] wegen, nochmals als die geeignetere. Die Allgemeinheit der Exponentialfunktion wird durch ihre Anwendbarkeit bei den Fresskurven der in Skinner's Versuchen benutzten Ratten angedeutet.

BOUSFIELD

SHORT ARTICLES AND NOTES

MUSCULAR TENSION RESULTING FROM GLARE

M. Luckiesh and Frank K. Moss

Since there is neither a system of absolute units to express the effects of glare nor a likelihood of establishing such a system, it is necessary to discuss the subject in terms of various arbitrary criteria. A glance at a representative bibliography of the subject (6) will indicate the diversity of methods used to appraise the effects of glare. In specific cases it is sometimes possible to select a criterion which will satisfactorily denote the relative degree or seriousness of a glare-condition. However, the significance of such a criterion is usually ambiguous for other situations. Hence a number of different methods for estimating the influence of glare are desirable and this introduction of an additional one is correspondingly justifiable.

In general, two different methods have been used for determining the deleterious effects of glare. In one of these, the subject of the experiment is asked to report on the unpleasantness or discomfort resulting from the presence of a glaring light-source. More reliable results are obtained if the experiment is so arranged that the subject compares the relative glare-effects from two light-sources in this respect. This method has the advantage of probably being the most sensitive one which has been devised. However, it leaves much to be desired, chiefly from the lack of adequate definition of the exact thing which the subject is to appraise. The second method involves measuring the reduction in the visibility of an object under various conditions of glare, the object itself having nothing to do with the glare-conditions. This method has the advantage of definiteness and fair precision. It is emphasized that the two methods described are not methods for appraising the same thing, but rather means for determining the effect of glare in terms of discomfort and reduced visibility, respectively. It has been established that a glare-condition which may be judged quite uncomfortable often produces no detectable reduction of visibility. Therefore, it follows that a glare-condition which appreciably reduces visibility is certainly an adverse condition for the eyes. It has also been shown (4) that the reduction in visibility by glare is practically entirely a function of the intensity of illumination at the eyes due to the glare-source, other conditions remaining constant. If the effect of glare is appraised upon a basis of discomfort, the intrinsic brightness of the glare-source becomes an influence in addition to the foregoing factor. A discussion of glare criteria and terminology is available elsewhere (3).

It is reasonable to assume that the estimation of the discomfort produced by glare represents an integrated reaction to several physiological and psychological factors. The advantage of the comprehensiveness of this criterion, however, is largely discounted by the extremely wide variations in the results which are characteristic of subjective methods of experimentation. In the following discussion an attempt has been made to appraise glare upon a basis of discomfort by means of data objectively obtained.

The tightening of the muscles of the hand, for example, is a common reaction to pain or discomfort. Therefore, it is conceivable that such a muscular reaction may be produced by glare. Whether or not this reaction is of sufficient magnitude to serve as a criterion for appraising glare is a matter to be determined experimentally. It is also uncertain how completely muscular tension in the hand indicates the presence of ocular discomfort. In view of the inherent relativeness of all glare measurements, this is not considered as an essential datum.

This investigation relating illumination intensity, preventable glare and nervous muscular tension formed one phase of an extensive investigation (5) which showed conclusively that the tension decreases considerably with increasing intensity of illumination for such a visual task as reading. The decrease continued with increasing intensity of illumination up to 100 foot-candles, the highest intensity used, and indicated that it would continue to decrease as the intensity was increased much further. It should be noted that the reading could be done at an intensity of illumination less than one foot-candle which emphasizes the limitations of threshold data in the study or practice of lighting and of seeing.

In the present research, the subjects were required to observe steadily a rotating test-object and to press a key at certain intervals as prescribed by the position of a detail of the test-object. The test involved extremely difficult visual work and the relatively easy mechanical task of operating the key. The task, as a whole, was of such an exacting nature that the subjects instinctively rested their fingers upon the knob of the key continuously during the test-period of ten minutes. It was thus possible to record changes in the pressure which the subjects unconsciously exerted upon the key. The changes in nervous muscular tension, as indicated in the fingers of the right hand which operated the key, were correlated with changes in the experimental variables of preventable glare and level of illumination. A detailed description of the test-object and auxiliary apparatus is given elsewhere (1).

The key (Figure 1) was so constructed that an auxiliary circuit was closed when the pressure upon it exceeded a certain predetermined value. This critical pressure was determined empirically for each subject and remained constant throughout the entire series of tests. The gap between the auxiliary contacts was so small that the closing of these contacts was not indicated to the subject by the sense of touch nor could they be seen since

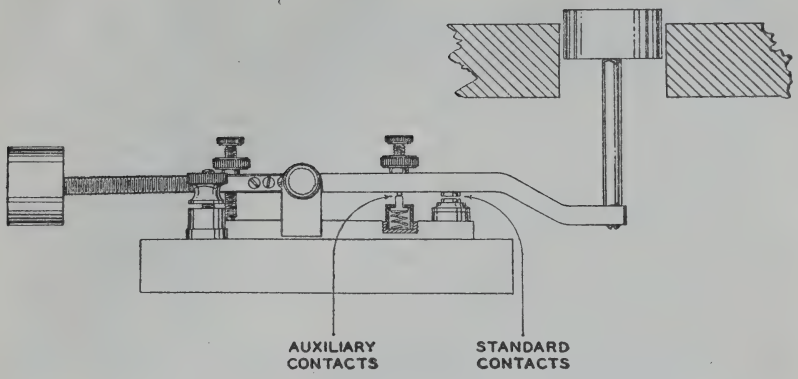

FIGURE 1

DETAILS OF THE KEY BY MEANS OF WHICH PRESSURE MEASUREMENTS OF NERVOUS MUSCULAR TENSION WERE TRANSMITTED TO THE RECORDING APPARATUS

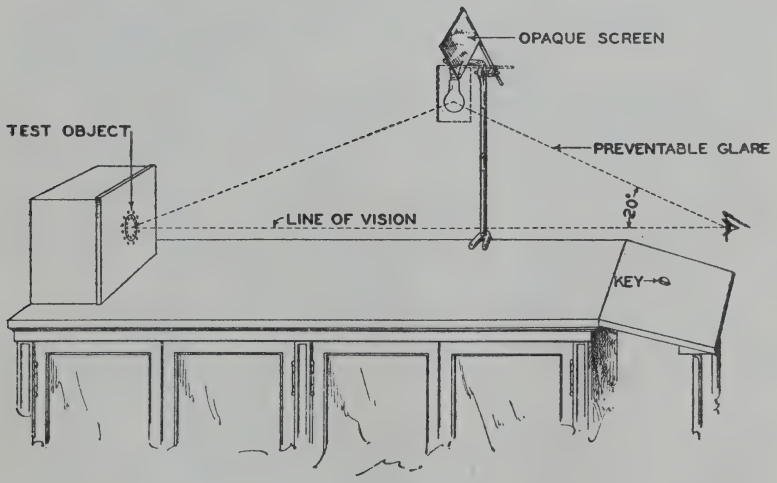

FIGURE 2

THE ARRANGEMENT OF THE APPARATUS

all parts of the key except the knob were concealed. The standard or ordinary contacts of the key were connected to electric counting devices which recorded the accuracy with which the visual task was performed. The auxiliary contacts were connected to an electromagnetic device which recorded the opening and closing of the auxiliary circuit upon the drum of a kymograph. However, the subjects were not aware of this arrangement.

Intensities of illumination of 1 and 5 foot-candles, respectively, were produced at the test-object and at the eyes by lamps placed as shown in Figure 2. Therefore, these lamps served as a source of illumination for the test-object and also as a glare-source in relation to the eyes. The direct light from the lamp to the eyes (preventable glare) was eliminated by an opaque screen to provide the "glareless" condition. Since this investigation was not concerned with the influence of the location of the glare-source within the visual field, this factor was held constant. The brightness of the glare-source and its angular position with respect to the line of vision are the same as used in a previous and extensive investigation (2) of the effects of glare upon visibility.

The subject was seated at the table indicated in Figure 2, with the index finger and usually the second finger resting naturally upon the broad knob of the concealed key. Since the weight of the hand was supported by the table, the fingers normally exerted but a slight pressure upon the key. As the test progressed, it was found that the subject unconsciously varied the pressure on the key and the auxiliary circuit was opened and closed in an irregular manner. The summation of the times the auxiliary circuit was closed is assumed to be an indication of nervous muscular tension and is so designated in this discussion. Intensity of illumination and preventable glare were the experimental variables, and other factors which might affect the performance of the subject were carefully controlled.

Measurements of indicated nervous muscular tension were made for intensities of illumination of 1 and 5 foot-candles, respectively, without preventable glare, and at an intensity of illumination of 5 foot-candles at the object and 5 foot-candles of preventable glare at the eyes. A series of 5 measurements arranged in an order to equalize the influence of learning were made for each of these three experimental conditions and for each of 10 observers. The results are summarized in Table 1.

These data indicate that the deleterious effects of the glare-source were comparable in magnitude to the effect produced by decreasing the level of illumination from 5 foot-candles to 1 foot-candle as appraised by the criterion of nervous muscular tension. It is also obvious that the results are significant when compared to their corresponding probable errors. An analysis of the data on a basis of accuracy with which the tests were performed indicated no differential between the three experimental conditions. This is additional evidence that the human seeing-machine is often able to operate

TABLE 1

THE INFLUENCE OF ILLUMINATION INTENSITY AND PREVENTABLE GLARE UPON INDICATED NERVOUS MUSCULAR TENSION

The values given for muscular tension denote the time in seconds during which the pressure unconsciously exerted upon the key exceeded a fixed value. The probable error of the mean is computed from the 10 individual values; E, by the formula $E_m = \frac{1}{N}\sqrt{\Sigma E^2}$

Subject	1 fc. on test-object No preventable glare		5 fc. on test-object No preventable glare		5 fc. on test-object 5 fc. preventable glare	
	Indicated muscular tension	Relative probable error %	Indicated muscular tension	Relative probable error %	Indicated muscular tension	Relative probable error %
1	117	16.2	94	12.7	231	27.5
2	121	21.2	63	18.5	131	14.1
3	228	9.0	138	23.4	292	15.3
4	170	8.4	109	18.0	160	18.4
5	298	7.9	77	9.4	200	11.4
6	153	6.2	150	31.1	154	9.2
7	95	16.6	61	20.0	119	16.2
8	236	27.2	68	22.5	255	22.2
9	130	4.9	75	19.1	102	10.8
10	103	9.6	41	11.5	101	17.6
Mean	154*	4.6	82*	6.2	163*	5.4

*Geometric mean

under severe handicaps without influencing the accuracy of its performance, but, doubtless, at a cost to it which at present is unappreciated or at least unmeasured.

The authors acknowledge the assistance of Mr. S. K. Guth in the accumulation of these data.

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A NOTE CONCERNING THE COMBINATION OF TEST SCORES

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With the increasing use of objective tests in combination with essay tests in large class sections, the problem of combining scores is again brought to attention. At the University of Chicago, the so-called comprehensive examinations in the four survey courses have consisted of objective and essay sections. The scores on the two sections are combined to give a final rating.

One widely accepted method of combining test scores is the addition of properly weighted standard scores (3, pp. 114-117; 2, pp. 118-121). Where unstandardized tests are used, and immediate results demanded, as is our case, the weighting cannot conveniently be based upon statistical procedures involving the use of multiple regression coefficients. Our weighting is arbitrary. The time the sections require usually gives a general basis for the weighting. Other factors may influence the weights assigned.

Standard scores (1, Formula 408) $z = \frac{X - M}{\sigma} = \frac{x}{\sigma}$ are difficult to compute

and to handle as both positive and negative numbers are included and as subtraction as well as division is necessary. Thurstone (4, p. 84) has shown, however, that as far as combination purposes are concerned, the scores expressed in standard deviation units are just as valid as are standard scores. The proof of this proposition follows.

$$T = w_1\frac{x_1}{\sigma_1} + w_2\frac{x_2}{\sigma_2}$$

$$T = \frac{w_1}{\sigma_1}X_1 + \frac{w_2}{\sigma_2}X_2 - \left(\frac{w_1M_1}{\sigma_1} + \frac{w_2M_2}{\sigma_2}\right)$$

$$T = \frac{w_1}{\sigma_1}X_1 + \frac{w_2}{\sigma_2}X_2 - C$$

$$U = \frac{w_1}{\sigma_1}X_1 + \frac{w_2}{\sigma_2}X_2$$

T = total score
w = weight
$x = X - M$
X = raw score
M = arithmetic mean of raw scores
σ = standard deviation of the distribution of raw scores
$U = T + C$

Then $r_{crit\ T} = r_{crit\ U}$ since the only difference between T and U is an additive constant C.

This procedure is simple to carry out. The value of w/σ may be computed, and each obtained score multiplied by it. With a calculating machine this is not a difficult task, even for a large number of cases. The other section or sections may be similarly treated, and the derived scores combined by simple addition. A convenient work sheet might be arranged as follows:

TEST _____ $s_1 = \frac{w_1}{\sigma_1} =$ _____ = _____

DATE _____ $s_2 = \frac{w_2}{\sigma_2} =$ _____ = _____

	Raw scores		Weighted scores in σ units			
	Obj.	Essay	Obj.	Essay	Total	Rating
Name	(1)	(2)	(3)	(4)	(5)	(6)
Adams, John Q.	X_{1a}	X_{2a}	$X_{1a}s_1$	$X_{2a}s_2$	$X_{1a}s_1 + X_{2a}s_2$	
Bryan, W. J.	X_{1b}	X_{2b}	$X_{1b}s_1$	$X_{2b}s_2$	$X_{1b}s_1 + X_{2b}s_2$	
Coolidge, C.	X_{1c}	X_{2c}	$X_{1c}s_1$	$X_{2c}s_2$	$X_{1c}s_1 + X_{2c}s_2$	
Darrow, C.	X_{1d}	X_{2d}	$X_{1d}s_1$	$X_{2d}s_2$	$X_{1d}s_1 + X_{2d}s_2$	
Total	ΣX_1	ΣX_2	$\Sigma X_1 s_1$	$\Sigma X_2 s_2$	$\Sigma(X_1s_1 + X_2s_2)$	

Checks: $s_1\Sigma X_1 = \Sigma X_1 s_1$
$s_2\Sigma X_2 = \Sigma X_2 s_2$
$\Sigma X_1 s_1 + \Sigma X_2 s_2 = \Sigma(X_1 s_1 + X_2 s_2)$

Now, of course, the scores X_1 and X_2 or X_1s_1 and X_2s_2 may differ greatly in magnitude even though $w_1 = w_2$. Thus X_1 scores might be 1000 or more

while the X_2 scores might be all less than 100. This discrepancy is of no statistical significance as the scores may be made into directly comparable magnitudes by the subtraction of the appropriate constant $\left(\frac{wM}{\sigma}\right)$.

Correlational work is frequently done with test scores, and scores recorded according to the sheet here outlined may be readily fitted into a simple formula for the product moment correlation coefficient.

$$r_{12} = \frac{\frac{\Sigma X_1 X_2}{N} - M_1 M_2}{\sigma_1 \sigma_2} \quad \text{(see 1, Formula 183)}$$

$$r_{12} = \frac{1}{N}\left(\frac{\Sigma X_1 X_2}{\sigma_1 \sigma_2} - \frac{N M_1 M_2}{\sigma_1 \sigma_2}\right)$$

$$X_1 = \frac{1}{w_1}(w_1 X_1)$$

$$r_{12} = \frac{1}{N w_1 w_2}\left(\frac{\Sigma w_1 X_1 w_2 X_2}{\sigma_1 \sigma_2} - \frac{N w_1 M_1 w_2 M_2}{\sigma_1 \sigma_2}\right)$$

$$= \frac{1}{w_1 w_2}\left(\Sigma \frac{\left(\frac{w_1 X_1}{\sigma_1} \cdot \frac{w_2 X_2}{\sigma_2}\right)}{N} - \frac{w_1 M_1}{\sigma_1} \cdot \frac{w_2 M_2}{\sigma_2}\right)$$

To use the notation of the data sheet:

$$r_{12} = \frac{1}{w_1 w_2}\left(\frac{\Sigma(X_1 s_1 \cdot X_2 s_2)}{N} - \frac{\Sigma X_1 s_1}{N} \cdot \frac{\Sigma X_2 s_2}{N}\right)$$

As $\frac{1}{w_1 w_2}$ usually equals $\frac{1}{2 \cdot 3}$ or $\frac{1}{1 \cdot 2}$ or some such simple number, this calculation is simple. The summation of the product term may be obtained in one summation operation by adding the products of each number in column (3) with its corresponding number in column (4) on the data sheet. It is the only difficult calculation to make, and with the use of such a calculating machine as the United, it is relatively easy to secure and to check. It may, of course, also be obtained with the ordinary Monroe or Marchant calculator, although the checking here is more difficult. The $\Sigma X_1 s_1$ and $\Sigma X_2 s_2$ have already been obtained in checking the data sheet (Totals, columns 3 and 4). Thus r may easily be computed.

Experimentation with the method here outlined has proved its value in ease of computation and in facility of checking.

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A NOTE ON THE USE OF BI-SERIAL R IN TEST RESEARCH

M. W. Richardson and John M. Stalnaker

Occasionally it becomes desirable to find a measure of correlation between two variables when the scatter diagram is based on a table with 2 x N class intervals. One variable is continuous and quantitative in the usual sense; the other is dichotomous. The usual bi-serial r, given by the formula $r_{bis}=\frac{M_2-M_1}{\sigma}\cdot\frac{pq}{z}$ (5, p. 247), is useful in certain special situations.

The derivation of this bi-serial r is based upon the assumptions

(1) that the regression is linear (2, pp. 127-128; 4, pp. 271-272),

(2) that the distribution of the dichotomized variable is normal (2, pp. 127-128; 4, pp. 271-272; 5, pp. 245-249), and

(3) that the variable underlying the dichotomous classification may be regarded as continuous (4, pp. 271-272; 5, pp. 245-249).

That this formula is applicable to all situations in which one of the variables is expressed in two categories has not been implied by any of the references cited (2, pp. 127-128; 4, pp. 271-272; 5, pp. 245-249). It seems that no statistician would venture to use "male" and "female" as the dichotomous variable.[1] Biologists might agree to ascribing a continuous nature to the sex variable, but they would hardly sanction the postulation of a normal distribution on this variable with its absurd connotation. In certain other situations, the fallacy in the application of this measure is somewhat less obvious. One example of such misuse which comes to our attention

[1]C. W. Odell (6, p. 278) makes this error. Also the use he ascribes to bi-serial eta is incorrect.

is the application of bi-serial r to the relation between an item response and total score on the test (or criterion measure) (1, pp. 366-367).

In this situation, the Y-variable is expressed as a set of test scores. The X-variable is simply "passing" or "failing" the individual test items. The bi-serial r was computed for each item. The r's for the various items are used, presumably, to indicate which items are most in agreement with the criterion scores. One may also presume that the bi-serial r was adopted for the reason that the significance of the difference between the mean scores of those passing the item and those failing it (M_2—M_1) must be interpreted in terms of the percentage "passing" the item (i.e., difficulty). Thus an item passed by 50% of the subjects may be equally as valid as an item passed by 10 (or 90) % of the subjects even though M_2—M_1 is smaller in the former case. Nevertheless, the fundamental assumptions of this formula do not justify this application. If the item response is not discontinuous, the efforts to objectify the response have been futile. Moreover, the assumption of normality of the test-response function for one item seems more than a little difficult to justify. Another point is worth noting. Usually test items are weighted unity for passing, and zero for failure to give the correct response. This situation is certainly not duplicated in the bi-serial r, where the values assigned to the test response (X_2 and X_1) shift with the percentage passing (p). If item weights were made to vary with the difficulty of the item, the weighting would be made on grounds other than those implied in the bi-serial r.

With these objections in mind, we wish to propose for item analysis a substitute bi-serial coefficient based on more suitable assumptions. If no assumption of continuity or normality is made about the X-variable, and if $X_2=1$ and $X_1=0$, we may write $r=\dfrac{M_2-M_1}{\sigma}\sqrt{pq}$ where M_2 is the mean value of Y for the X_2's, and M_1 is the mean value for the X_1's, p is the percentage of measures in the X_2 category, and q is equal to $1-p$.

This formula may be simply derived thus:

Assume $X_2-X_1=1$ (i.e., all items will be weighted unity or else discarded).

$$b_{yx} \text{ (slope of the regression line)} = \frac{M_2-M_1}{X_2-X_1} = M_2-M_1$$

$$b_{yx}=r\frac{\sigma_y}{\sigma_x}$$

$$\sigma_x=\sqrt{pq} \qquad \text{(See 3, Formula No. 373)}$$

$$r=\frac{M_2-M_1}{\sigma_y}\sqrt{pq}$$

In actual application of this formula to item analysis, the validity of an item depends upon the difference of the mean criterion score of those who pass and those who fail the item and the difficulty of the item. Of course, p and q are interchangeable. The formula suggested has thus the essential characteristics of the bi-serial r, and none of its disadvantages from the standpoint of underlying assumptions. It is recognized that the relationship between two bi-serial r's is approximately linear for a wide middle range of values of p. The proposed coefficient presents no practical difficulties in computation, and what is more important, the data for wholesale computation are easily adapted to Hollerith card set-up.

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A CONDITIONED CHANGE IN VOCAL PITCH[1]

Harold C. Taylor

The principle of dynamogenetic irradiation would demand that the somatic and visceral tensions induced, for example, by an electric shock, should spread rather widely throughout the organism and influence, among other effectors, certain muscles of the vocal cords, a progressive tautening of which raises the pitch of vocalization. A word spoken during electric shock should therefore be at a higher pitch than one spoken during no shock. If this be true, then the ordinary principles of conditioning indicate further that the heightened-pitch response normally evoked by shock should be transferable to some such neutral stimulus as a light. The following experiment was designed to show the effect of electric shock on the pitch of vocalization and to condition that effect to a light.

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[1]This work was performed in Professor Clark L. Hull's research unit of the Institute of Human Relations, Yale University. The writer is indebted to Dr. Hull for useful suggestions.

Apparatus and Procedure

The pitch-recording apparatus, screened from the subject, was a kymograph and tambour set-up described elsewhere (1). An ordinary inductorium with interrupter and dry electrodes was used to produce a rather strong shock on the right wrist of the subject. On a table directly in front of the subject were two 6-volt light bulbs, exactly alike, mounted about 3 inches apart. Stimulation occurred every 38.5 seconds throughout an experimental period, and consisted of the left light (L); the right light (R); or the right light (R_s) followed after 0.4 second by electric shock which lasted approximately 0.4 second. Eight male students of Yale University were used as subjects. The subject's sole instructions were to say the word "sat" into the mouthpiece every time he saw either light. The following pattern of stimuli was repeated five times, with a pause of about 5 minutes at the end of the second and fourth series, when the kymograph paper was changed. Only the vocalizations following those stimuli which are boldfaced were recorded.

Series 1 **L L L L R_s** L **R_s** L L L **R_s** L L **R_s** **L L L L R**

Series 2, 3, 4, 5 **L L L L R_s** L **R_s** L L L **R_s** L L **R_s** **L L L L R**

Results

The clearest evidence of the extent to which shock raised the pitch of the voice is found by comparison of the pitch of the responses following the first four L's of Series 1 with those following the four R_s's of the same series. Briefly stated, those averages for the eight subjects are 155.85 d.v. per second and 183.27 d.v. per second, respectively. The mean rise in pitch induced by shock is thus 27.42 d.v. per second, which is 13.7 times its probable error. All subjects clearly show the tendency.

TABLE 1

A Conditioned Change in Vocal Pitch

Subject	Mean pitch of response to neutral light	conditioned light
FGH	223.20	224.45
JJC	151.70	155.17
EHJ	158.93	169.82
RPW	184.97	194.18
WHG	173.22	176.56
PAM	178.00	179.84
HJF	192.98	193.64
RWP	142.00	142.82
Mean	174.02	178.03
Diff./P.E.$_{diff.}$	4.43	

As a measure of the extent to which conditioning of the high-pitch response had taken place, the mean pitch of the final R's (five such responses by each subject) were compared with the mean pitch of the immediately preceding L's (20 such responses by each subject, 4 in each of the 5 series). These means are given for each subject in Table 1. It will be seen that all subjects give a higher-pitched response to the conditioned right light than to the neutral left light. While this difference is small in the case of some subjects, the mean difference of the group is 4.4 times its probable error. As is customary in such work, all pitches were reduced to logarithms before probable errors and differences were computed.

Summary and Conclusions

1. The pitch of vocalization is higher during electric shock than during no shock.

2. The rise in pitch induced by shock can easily be conditioned to a light.

3. It may be deduced from (1) and (2) that any vocalization ordinarily occurring concomitant with marked activity of either the striped or the smooth musculature should be pitched higher than other vocalizations; and that through conditioning, the higher pitch of such vocalizations should obtain even in the absence of the usual concomitant activity. Thus, for example, words usually spoken in situations involving emotion should be pronounced at a measurably higher pitch than other words; and through conditioning, the higher pitch should remain characteristic of such words even when spoken in non-emotional situations.

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THE EFFECT OF BOUNDARIES ON SPACE ESTIMATION[1]

Eli S. Marks

Problem

One of the most important contributions of Gestalt psychology to the study of perception is the concept of dynamic organization of the visual field. This organization is essentially into figure and ground. Even such

[1]This study was supervised by J. P. Seward, Jr., in the Psychological Laboratories of Columbia University.

a simple affair as two parallel lines may be said to form a figure. Köhler (2) gives an explanation of this in terms of a "force of attraction" between the lines. This force may be considered a convenient name for some process, possibly a tendency toward a change of convergence of the eyes, similar to the tendency which has been proposed in order to explain stereoscopic effects. The concept of a "figural force" is, however, by no means a new one. It has appeared not infrequently in the literature on illusions of space estimation. Ladd and Woodworth (3, p. 442), at the same time citing earlier, similar formulations, suggest that "when a point is apprehended as part of a compact figure, it becomes attracted, or displaced, toward the center of that figure." The present experiment is perhaps most closely related to previous studies of these illusions, and may be considered as a further contribution to the topic.

The hypothesis of a "figural force" may be tested by varying the intensity and direction of the assumed force and observing the changes, if any, in the estimation of the sizes of the spatial extents involved. In the case of parallel lines Köhler (2) indicates that their appearance of cohesiveness, of forming a figure, is increased the closer the lines are brought to each other. Decreasing the distance between two parallel lines should thus increase the "figural force" between them. If we bound a spatial extent with two figures as in Figure 1, B and C, we can keep the objective size of the extent constant while varying the figure and ground relations.

Procedure

Three sets of cards were used, as in Figure 1, A, B, and C. The spatial extents bounded by the lines marked 1 and 2 were compared with a standard extent. The standard extent was 50 mm. in width and was bounded by two vertical lines, as in Figure 1, A. The various extents may be referred to as intervals 1-2, 3-1, and 2-4, the numbers referring to the lines bounding them.

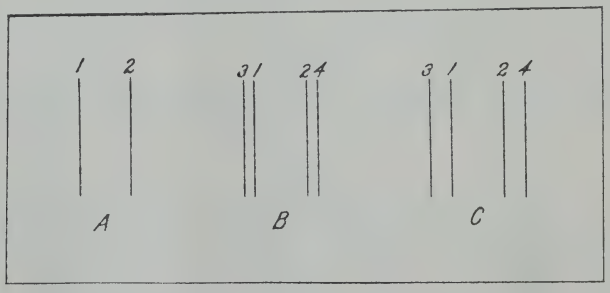

FIGURE 1

Illustrating the Three Series Used in the Experiment

The method of constant stimulus differences was used and the limens for the estimation of interval 1-2 in each series (A, B, and C) were determined. The widths of the variable interval 1-2 were the same for all three series. Five variables were used in each series: 46, 48, 50, 52, and 54 mm. wide. Series A (Figure 1, A) was bounded by two lines, 1 and 2, only. Series B (Figure 1, B) had its middle interval bounded by intervals 3-1 and 2-4, each 10 mm. wide. Series C (Figure 1, C) had its middle interval bounded by intervals 3-1 and 2-4, each 20 mm. wide. Differences between the limens of the three series may be taken as a measure of the effect of the different boundaries on the perception of interval 1-2. The effect of fluctuations in perception must be taken into account in drawing such a conclusion, but large and consistent differences may be safely attributed to the boundary conditions. Care was taken to control time and space errors, practice, fatigue, and the effect of directions to the subjects.

Each card was exposed with the standard for a period of 2 seconds through rectangular apertures 15 cm. wide by 7.5 cm. high. The standard appeared half the time on the right and half the time on the left. The order of presentation was ABC for 15 cards, CBA for the next 15, and so on. Each card appeared once in every group of 15 comparisons: thus practice and fatigue effects were practically the same for all cards. The order of presentation was reversed for the last part of each sitting. For half the judgments the subjects were asked whether the standard was broader than, narrower than, or equal to the other card. For the other half, judgments referred to the variable. For convenience all judgments have been recorded under the three headings: "variable broader than standard," "variable narrower than standard," and "variable equal to standard."

Two subjects, Phi and Haz, made 1500 comparisons each; i.e., 500 in each series and 100 for each variable. At each half-hour sitting 150 comparisons were made, and two sittings, one hour apart, were given at the same time each day for five days. The order of presentation was varied frequently to avoid memorization of the order by the subjects.

Results

The numbers of judgments for each variable of broader, narrower, and equal are given in Table 1. Table 1 thus gives the percentage of judgments directly, since the total number of judgments for each variable was 100. It also gives the limens, the measures of precision (h), and the standard deviations. These were calculated by the constant process with the formulas and Urban's table of constants given by Brown and Thomson (1).

The results show a fairly regular decrease in narrower and increase in broader judgments from the 46-mm. to the 54-mm. variable. There is only one minor inversion of the first order.

Table 1 indicates a decided tendency to estimate the B and C series as broader than the A, the B to a greater degree than the C. There are more

TABLE 1

RESULTS OF COMPARING THE A, B, AND C VARIABLES WITH A STANDARD OF 50 MM.

Subject	Judgment	Series	Length of variable					Limen	*h*	*SD*
			46	48	50	52	54			
Phi	Variable broader	A	1	4	45	92	100	50.13	1.98	.50
		B	14	56	100	100	100	47.51	1.74	.55
		C	1	22	80	100	100	48.93	2.26	.47
	Variable narrower	A	99	93	46	1	0	49.77	2.16	.48
		B	84	38	0	0	0	47.35	1.82	.52
		C	99	74	13	0	0	48.72	1.01	.70
	Variable equal	A	0	3	10	7	0			
		B	2	4	0	0	0			
		C	0	3	5	0	0			
Haz	Variable broader	A	2	1	15	72	97	51.16	1.57	.56
		B	18	69	97	99	100	47.33	1.75	.53
		C	1	7	45	94	100	50.03	1.96	.51
	Variable narrower	A	96	95	44	9	0	49.81	1.62	.56
		B	68	11	2	0	0	46.57	1.98	.53
		C	97	78	15	0	0	48.29	3.21	.40
	Variable equal	A	2	4	41	18	3			
		B	16	20	1	1	0			
		C	3	15	40	6	0			

broader and fewer narrower judgments for B than for C, and for C than for A, consistently. The limens show the same tendency. The B limens are lower than the corresponding C limens and the latter are lower than the A limens. Assuming that lengths showing equal numbers of judgments of each kind are subjectively equal, B and C must be overestimated as compared with A, since lengths in these series are judged equal to longer lengths in the A series. In the same way, B must be overestimated as compared with C.

Earlier experiments were conducted with more subjects and fewer judgments, using only the B and C series. The standard used in the preliminary work was of the same type as series B. The results indicate that B was overestimated as compared with C. Table 2 gives the limens for a group of 15 subjects, Group I, giving 200 judgments each, or 20 judgments per variable and a group of 10 subjects, Group II, giving 100 judgments each, or 10 judgments per variable. Limens were obtained by linear interpolation.

TABLE 2

GROUP LIMENS FOR SERIES B AND C COMPARED WITH STANDARD OF TYPE B

Group	Series	Variable broader	Variable narrower
I	B	51.9	50.8
	C	54.4	52.0
II	B	51.8	50.2
	C	53.0	52.3

CONCLUSIONS

From the results we may conclude that A appears narrower than C, and C narrower than B. The preliminary experiment indicates that this phenomenon is not confined to the two subjects used, but, so far as the B and C series are concerned, also applies to other groups.

Three possible effects of boundaries on space estimation have been suggested. It is possible that the interval tends to become confused with the boundary and thus is overestimated. If this were the case, A should appear narrower than B and B narrower than C. C, however, appears narrower than B.

Secondly, it has been suggested that primarily the whole interval from line 3 to line 4 (Figure 1, B and C) is perceived, but with larger or smaller pieces subtracted from it. If this were the case, A should appear wider than B or C since there is nothing subtracted from its total width.

Discarding the above theories, the interpretation suggested in the introduction proves agreeable with all the facts. If a force exists across the boundary figures, intervals 3-1 and 2-4 in Figure 1, B and C, these boundaries should tend to move together. There would thus be a tendency for lines 1 and 2 to be displaced outwards towards 3 and 4 respectively. The displacing tendency would presumably be proportional to the strength of the figural force. Intervals 3-1 and 2-4 of series C form less cohesive figures than the corresponding intervals of series B. Consequently lines 1 and 2 of series B will tend to be displaced farther outwards than lines 1 and 2 of series C, due to stronger figural force. Interval 1-2 of series B should therefore appear wider than interval 1-2 of series C. This is actually so.

Series A has no bounding figures and therefore lines 1 and 2 should experience either no displacing tendency at all or an inward displacement (assuming interval 1-2 to form a figure here). (These figure-ground relationships were not empirically determined, but are assumed for purposes of interpretation.) Interval 1-2 of series A would thus appear narrower than the corresponding intervals of B and C. This also fits the facts.

To conclude that there is strong evidence for an explanation in terms of "figural force" seems justified.

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THE EFFECT OF FORM ON COLOR CONTRAST[1]

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Wertheimer (5) and Benary (1) have tested the effect of designs on relative brightness and have shown that two identical gray triangles may not only differ in brightness when set into certain black and white designs, but that, contrary to the traditional laws of contrast, the triangle surrounded by the greater area of white may actually appear to be the lighter. Mikesell and Bentley (4) confirmed Benary's results under more carefully controlled conditions, but differed from him in their interpretation, contending that the hypothesis of "configuration" and "inherence in the figure," as here applied, is not clear, consistent, or adequate. Jenkins (2) extended their work and interpreted his results in terms of the relative "inherence" of the two grays.

Benary's observers, when shown a blue design on a yellow background, reported the excluded gray as bluish and the included gray as yellowish. Jenkins attempted to verify this, but with inconclusive results. In the present experiment we planned to test these results, using green figures on red grounds.

Procedure

On a chart (see Figure 1) having a green figure on a red ground three identical gray triangles were placed so that one was surrounded entirely by red, a second was placed in the figure, and the third alongside the figure. The immediate border conditions of the "included" and "excluded" triangles were identical. (These terms refer to the objective, rather than the subjective, relationships of the triangles.) It will be noticed, however, that the area immediately surrounding the included gray contains a greater proportion of red than that surrounding the excluded gray.

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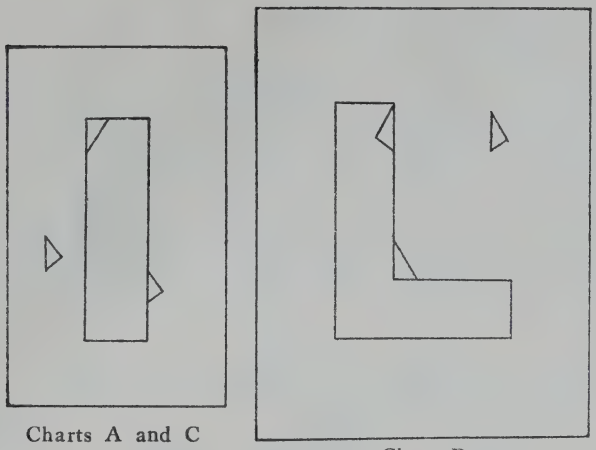
[1]This study was supervised by J. P. Seward, Jr., in the Psychological Laboratories of Columbia University.



Charts A and C

Chart B

FIGURE 1

DESIGNS USED IN THE PRESENT EXPERIMENT

Three charts were used, A, B, and C; 11″ x 18″, 17″ x 22″, and 14″ x 15″. On Chart A the figure was an upright rectangle, 10½″ x 3″; on B, an "L" that would just fit in a rectangle 9″ x 12″, each arm being 3″ wide; on C, an upright rectangle 10″ x 3″. The grays were right triangles, 2½″ x 2″ x 1¼″.[2]

In addition to the charts, two matching cards were built to present a variety of grays deviating by slight degrees toward red or green. Two 19″ x 19″ neutral gray cards were perforated with sixteen 2″ x 2″ openings in four rows of four, in which were displayed the red-gray and green-gray colors scattered at "studied random" except for a checker-board alternation of red-grays and green-grays. The card could thus be presented with any side uppermost without the subject's being aware that it had been turned. The red-gray and green-gray series were made by painting neutral gray cards with a series of mixtures of Devoe and Reynolds Tempora colors, formed by adding small amounts of red or green to gray paint. The paints used were subjectively matched in brightness. Each series was numbered from 1 to 20 in order of saturation.[3] From these series cards

[2]The charts were constructed of Milton Bradley's colored papers and colored pasteboards, the paper figures rubber-cemented to the boards. The green was Milton Bradley's "Green," Plate 18, L-11 of *The Dictionary of Color* (3); the gray was "Neutral Grey Dark," Plate 45, 4-A. The red on A and B was "Red," Plate 6, L-6; that on C was "Bright Red," Plate 3, L-6.

[3]The reddest card was closest to Milton Bradley's Plate 44, D-4; the greenest, to Plate 29, H-4.

were selected varying by fairly uniform, almost imperceptible steps to make up the two matching cards.

In a dark room one of the charts and one matching card were displayed, lighted by a single 50-watt Mazda daylight bulb in a silvery parabolic reflector at one side. The subject was 9 feet away. The matching card was displayed about 10 seconds before a chart was presented. The order of display of A, B, and C, and the side presented uppermost, were rotated; likewise the two matching cards and their sides. The subject was instructed by a set verbal formula to match first the detached triangle, then the other two, one at a time, with the matching card, and finally to give a subjective description of the difference between the last two triangles. These were so

TABLE 1

MATCHINGS AND JUDGMENTS OF TRIANGLES ON CHART A

Subject	Matchings			Judgments of redder
	Detached	Excluded	Included	
1	G-15	R- 9	R- 9	Included
2	G- 5	R- 3	R- 5	Included
3	G-10	G-10	R- 2	Included
4	G-20	G-12	R- 8	Included
5	G-10	G-19	R- 8	Included
6	G-12	R- 4	R-12	Excluded
7	G-13	G- 7	R- 5	Included
8	G- 9	R- 5	R-13	Included
9	G- 3	G- 3	R- 7	Same
10	G- 5	G- 5	R- 9	Included
11	G-19	G-12	G- 4	Included
12	G-14	G- 8	R- 4	Included
13	G-15	G- 7	R- 9	Included
14	G- 3	R- 3	G- 3	Included
15	G- 9	G- 5	G- 7	Included
16	G- 8	R-12	R-19	Included

TABLE 2

REPETITION OF CHART A WITH THE FIRST EIGHT SUBJECTS

Subject	Detached	Matchings Excluded	Included	Judgments of redder
1	G- 7	R- 3	R- 9	Included
2	G- 7	R- 7	R- 9	Included
3	G-10	G-12	R- 8	Excluded
4	G- 3	G- 7	R-11	Included
5	G- 5	G- 7	R- 3	Included
6	G- 8	R- 2	R-12	Included
7	G- 3	G- 7	G- 5	Included
8	G- 5	R- 5	R- 7	Included

TABLE 3

MATCHINGS AND JUDGMENTS OF TRIANGLES ON CHART B

Subject	Matchings			Judgments of redder
	Detached	Excluded	Included	
1	G- 5	R- 5	G- 9	Included
2	G- 3	R- 9	R-13	Included
3	R- 1	R- 8	R- 2	Excluded
4	G- 3	R-15	R-12	Same
5	G- 3	R- 5	R-11	Included
6	G- 2	R-12	R -2	Included
7	G- 5	G- 7	G- 7	Included
8	G-11	R- 7	R- 7	Included
9	G- 3	R-17	R-17	Same
10	R- 3	R- 9	R- 9	Included
11	G-15	R-17	R-19	Included
12	G- 8	R- 2	R- 4	Included

TABLE 4

MATCHINGS AND JUDGMENTS OF TRIANGLES ON CHART C

Subject	Matchings			Judgments of redder
	Detached	Excluded	Included	
1	G- 8	R- 2	R-14	Included
2	G- 3	R- 5	R- 9	Included
3	G-20	G-14	G- 3	Included
4	G-19	G- 2	G-20	Excluded
5	G- 2	G- 2	R- 3	Included
6	G-14	G- 4	R- 2	Included
7	G- 7	G- 3	R- 2	Included
8	G- 5	G- 5	R- 3	Included
9	G- 3	G- 7	R-12	Included
10	G- 7	G- 7	R- 9	Included
11	G-20	G-19	R-17	Included
12	G-12	G- 4	R- 8	Included

mentioned in the directions as to suggest nothing as to inclusion or exclusion. The subject was allowed to make slow, careful judgment; thus no attempt was made to control time of exposure, eye-movements, after-images, or adaptation.

Sixteen subjects, 12 men and 4 women, ranging in age from 20 to 50, reported on Chart A, and 12 of them on Charts B and C. Half of the subjects reporting on Chart A repeated the test on the same chart for the determination of reliability.

RESULTS

Tables 1 to 4 show the results of the matchings and judgments of the individual subjects. In all four tables the numbers used in the first column

TABLE 5

SUMMARY OF RESULTS COMPUTED FROM INDIVIDUAL JUDGMENTS AND MATCHINGS

	Items	Charts A	A†	B	C
(1)	% of judgments I redder* than E	87	87	75	92
(2)	% of judgments E redder than I	6	12	8	8
(3)	% of judgments I and E the same	6	0	16	0
(4)	Average matching score‡ of D	G-10	G-6	G-5	G-10
(5)	Average matching score of E	G-3	G-2	R-8	G-5
(6)	Average matching score of 1	R-6	R-7	R-7	R-5
(7)	% of subjects matching D as green	100	100	83	100
(8)	% of subjects matching E as green	62	50	8	83
(9)	% of subjects matching I as red	81	87	83	83
(10)	% of subjects matching I redder than E	81	100	33	92
(11)	% of subjects matching E with green and I with red	50	37	0	66
(12)	% of judgments agreeing in direction with matchings	68	87	50	100

*"Redder" is used here, not only in the literal sense, but also in the sense in which G-2 is redder than G-10.

†Repetition of A with first eight subjects.

‡Treating the green series as a minus series and averaging algebraically.

refer to the same individuals. The next three columns give the numbers of the green-gray or red-gray squares with which the detached, excluded, and included triangles were matched, respectively. The last column indicates whether the excluded or included triangle was judged the redder.

Table 5 summarizes the chief relationships discovered. Items (1) to (3) show that the majority of subjective judgments indicated that the included triangle was seen as redder than the excluded. Items (4) to (6) show that on Charts A and C, while the matchings gave the detached triangle the highest average greenness, they gave the included triangle a higher average redness than the excluded.

On Chart B the average redness of the excluded triangle was slightly higher than the included. Items (7) to (9) show that, while the detached triangle was almost always matched as green, on Charts A and C the excluded triangle was matched as green more often than red, though on Chart B the reverse was decidedly true, and that on all charts the included triangle was most frequently matched as red. Item (10) shows that on Charts A and C, but not on B, the subjects almost always matched the included triangle as redder than the excluded. Item (11) shows that on Charts A and C, but not on B, the excluded and included triangles were matched as often as not on opposite sides of the color line, with the included triangle on the red side. The reverse situation occurred only once. Item

(12) shows the extent of agreement between judgments and matchings.

A comparison of the first and second tests of eight subjects with Chart A gives some indication of the reliability of the matchings and judgments. Seventy-five per cent of the second judgments and 87% of the second matchings were in the same direction as the first. The second matchings were all on the same side of the color line as the first.

Care was taken not to suggest to the subjects how to perceive the charts. From their comments it was possible to gather some idea of how the triangles appeared to them. Practically all the subjects used the phrase "in the green" to describe the included triangle, and "in the red" to refer to the detached triangle, if they referred to it. But in describing the excluded triangle they usually said "at the bottom (left) (right)" and sometimes "up against the green." Their further references to it might lead one to think that its relations are ambiguous; that it is not taken as part of the figure, nor as part of the background, nor as alone, but as somewhere between these relations. Questioned after the test, several said that it was not hard to imagine it as an appendage on the figure of another material, as a pump handle is part of a pump. Their comments indicated that in Chart B the excluded triangle joins with the figure more easily than do the excluded triangles in the other charts; it seems a sort of beveled corner of the L. Herein may lie one explanation of the discrepant results obtained with this chart.

Interpretation

The results of the experiment are qualitative and tentative, as it was done without elaborate apparatus for controlling a number of variables. Yet the results on Charts A and C corroborate with red and green what Benary found with blue and yellow, and are so consistent that they may be of interest to anyone attempting to do the same thing in a more thorough way. As pointed out, such results seem difficult to explain in terms of color contrast alone. Several explanations of the comparative redness of the included triangle may be proposed:

1. It is seen as part of the figure, which creates an expectancy that it will be green. This makes its departure from the expected green more striking and may induce a tendency toward opposition. The excluded triangle is not part of the figure necessarily, there is less expectancy that it should be green, and so its departure from greenness in less striking.

2. The eye behaves differently at the two borders between the green and the included gray and between the gray and red. The eye sweeps across the border that "should not be there" with greater ease than across the border that should be there.

3. Jenkins (2) suggests that the excluded triangle may be perceived as a more or less independent figure and as such may resist contrast effects. It is difficult to say whether the ambiguous relationships of the excluded

triangle reported by subjects in the present experiment are in harmony with this interpretation.

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APPARATUS

AN IMPROVED FORM OF THE GULLIKSEN TACHISTOSCOPE[1]

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In a recent issue of this journal Gulliksen (1) described a new tachistoscope which has a number of points of superiority over any older type of exposure apparatus. Sometime ago the writer constructed and used a replica of the Gulliksen tachistoscope. Later, in order to obtain increased flexibility and ease of operation, another tachistoscope was constructed which maintained the same fundamental design but included a number of new features. In operation this seemed a sufficient improvement over the earlier models to merit description.

The essential feature of Gulliksen's tachistoscope is that a flat metal plate pushes the bottom one of a stack of cards through a narrow opening into an exposure window. This pusher foot slides in a groove and is mounted on an oscillating lever driven by an eccentric cam. Since the pusher foot is thinner than a card it can move only one at a time. An additional safeguard is the width of the opening (less than the thickness of two cards) through which the card is pushed. Thus cards are certain to be presented singly in any order in which they have been stacked; the length of a series is determined solely by the experimenter's wishes; and any type of material can be used which is drawn or printed on cards. The timing both of interval and of presentation can be controlled by gearing, shutters, and so on, to suit the individual needs of any experiment. Either very short or very long presentations are possible, and the time of presentation is independent of the interval between successive cards.

The principal changes embodied in the new model are: (1) the introduction of a magnetic clutch between the motor and the eccentric drive of the pusher foot; (2) the addition of a built-in interval timer which, in connection with the magnetic clutch, greatly increases the range and flexibility of timing possibilities; and (3) the substitution of thin cards for the heavy fiber ones previously employed.

The magnetic clutch is similiar to that described by Renshaw and Hampton (2). A 1/100th H.P. General Electric synchronous motor running at 1800 r.p.m. is connected to a 30-1 worm reducing unit. An electromagnet

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[1]This apparatus was constructed in the Psychological Laboratory of The Ohio State University. To those who are acquainted with the Laboratory it is unnecessary to state that any piece of apparatus constructed there owes a great deal to the technical genius of Mr. I. L. Hampton, the departmental technician. I wish to thank him for his many valuable suggestions both in design and construction.

is built on to one end of the shaft carrying the worm gear. This runs continuously. When the 6-v. circuit through the interval timer is closed a 110-v. D.C. circuit is completed through the magnet, which then picks up the armature and sets it into rotation. The armature is geared in a ratio of 1 to 2 to the main drive shaft, giving the drive shaft a speed of 30 r.p.m. This shaft carries the cams which remove the already exposed card and present the new one. The motor and magnet run continuously, the drive shaft only when the magnetic clutch is energized. This discontinuous operation of the drive shaft allows any pattern, either regular or irregular, of presentation times, as a new card is exposed every time the magnet is energized.

The interval timer is a simple bakelite disc on which are placed five concentric rows of contact points. Individual contact fingers and switches allow any one of these five circuits to be closed. The period of the disc is 12 seconds. The five different time intervals, corresponding to the five rows of contacts, are 2, 3, 4, 6, and 12 seconds. These are the intervals between successive presentations. The exposure time is independent of the interval and is separately controlled by a cam on the drive shaft.

In addition to these five intervals, the opportunity is provided for greatly increasing the timing possibilities. This may be done in several ways: (1) changing the gear ratios in the chain of gears driving the interval timer would change all of the present intervals; (2) with all the circuits of the built-in interval timer open, any other timer may be connected directly to the relay; (3) in a similar fashion, a contact key may be substituted for the interval timer, allowing the experimenter to close the circuit and present a new card whenever he wishes. Such a range of intervals was impossible with the older models.

The contact on the interval timer merely starts the drive shaft in rotation. A contact switch operated by the drive shaft itself keeps the circuit through the magnetic clutch closed until the drive shaft has made one complete revolution. As the speed of rotation of the drive shaft is 30 r.p.m., the minimal interval between successive presentations is two seconds. During this period one cam removes a support from underneath a card which has already been exposed, allowing it to drop into a storage hopper. The support is immediately returned to catch the next card which is falling into the exposure window from above. The pusher foot then moves backward, picks up the next card, and pushes it out almost far enough to be pulled into the exposure window. The drive shaft then stops until such time as the circuit through the clutch is again closed, when the whole cycle is repeated.

The cards used in both machines previously constructed consisted of heavy, closely packed fiber 1/16 in. thick. These were rigid enough to jam the apparatus if they became warped; and care was necessary to prevent this

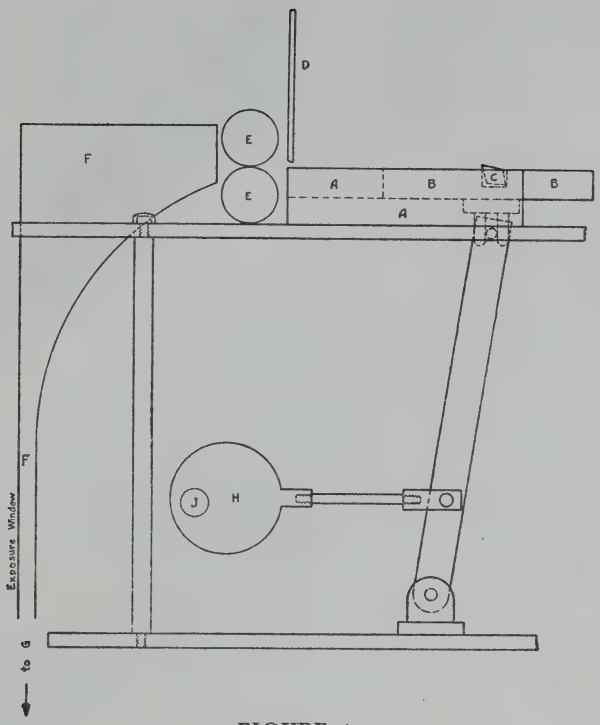

FIGURE 1

SCHEMATIC DIAGRAM OF PART OF TACHISTOSCOPE

A—fixed base on which stack of unexposed cards rests
B—sliding plate underneath stack of cards. (Both *A* and *B* are machined to a uniform flat surface.)
C—hardened steel piece set into *B*, and raised slightly above the surface of *B*, to catch the back edge of the bottom card
D—hardened steel guard to prevent more than one card from being pushed forward at a time
E—two wooden rollers which run continuously and which pull the cards into the top of compartment *F*
F—compartment in which cards are exposed. Gravity turns the cards into an upright position and carries them down to the exposure window
G—storage hopper for exposed cards (not shown in the diagram). After exposure the cards are dropped into G
H—eccentric cam which drives *B*
J—main drive shaft which rotates only when the circuit through the magnetic clutch is closed

condition, the cards being kept under considerable pressure and free from moisture. As an additional disadvantage, the noise of such cards falling into place, and then falling out of the exposure window and into the storage hopper, was not inconsiderable. Lastly they were rather expensive. To avoid these difficulties it was decided to use lighter cards of a hard-finished white ledger stock, .008 in. thick.

The employment of such thin cards has been very satisfactory. All of the objections to the thicker cards are overcome. Since the weight of half a dozen cards is sufficient to keep any card flat, warping no longer presents a problem. The disturbance of noise is very materially reduced because of the lightness of the cards. And such cards cost only about one-tenth as much as the fiber ones. The compactness of the thinner cards is an added feature. A magazine 3 inches high is adequate for a series of 300 cards, whereas previously that many cards required a magazine over a foot and a half high.

The lighter cards made certain changes in the mechanism for pushing out the bottom card necessary, as it was obviously impractical to attempt to use a pusher foot of only some .005 in. in thickness. The device employed can be best illustrated by a diagram. The stack of cards rests on *A* and *B* (see Figure 1), which are machined to a uniform flat surface except at the extreme front edge of *A* which is some .003 in. higher than the remaining surface. Set into the sliding foot (*B*) is a hardened steel piece (*C*) which can, by adjustment and locking screws, be made to project above the surface of *A* and *B* for any desired distance within a range of approximately an eighth of an inch. In the present model it is set to the project slightly less than the thickness of a single card, probably about .005 to .006 in. As the pusher foot moves forward underneath the stack of cards this slightly concave projection carries the bottom card forward with it.

The size of the opening at the front through which the cards are pushed is controlled by a piece of flat hardened steel (*D*), which can be raised or lowered and locked in any desired position. For the cards now in use the opening, as in the earlier model, is set so that one card can pass through while it is impossible for two cards to be pushed through at the same time; the clearance is about .010 to .012 in.

Immediately in front of this opening are two wooden rollers running against each other. These take hold of the card as soon as it is pushed into them and pull it into the top portion of the exposure compartment. Spring bearings on the top rollers allow for various thicknesses of cards while maintaining a definite pressure on all cards. The rollers are labelled *E* in the accompanying drawing; the exposure hopper *F*. After being pulled into the top of the compartment, the cards are turned by gravity and dropped into the exposure window.

A light shutter prevents the subject from seeing one card drop out of the

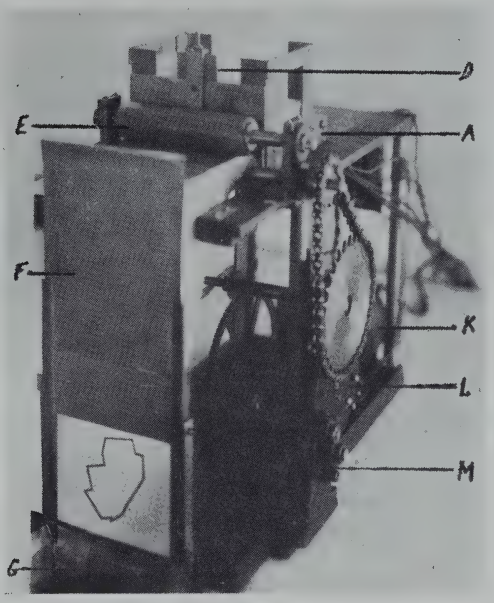

FIGURE 2

PHOTOGRAPH OF TACHISTOSCOPE

A, D, E, F, G, same as in Figure 1
K—1/100 H.P. General Electric synchronous motor
L—Interval timer disc
M—Housing for magnetic clutch

exposure window and the next one drop into position. It is controlled by a cam on the main drive shaft. The subject views the exposure window through an aperture in a cardboard screen. Behind the screen are placed two 10-watt lights. These are illuminated after a given card is in place, and turned off before the card drops out of the exposure window. This period of illumination constitutes the exposure time. In order to minimize the lag of the lights in attaining their full brightness, a resistance of some 5000Ω is connected in series with them. This resistance keeps the filaments at a cherry-red glow but does not provide sufficient light for the subject to see the movement of the cards. During the exposure period the cam on the drive shaft completes a shunt around the resistance, thus bringing the lights to maximum brightness very rapidly.

Although light shutters only have been used with this apparatus, any magnetic or mechanical shutter could be used and controlled in the same manner by the apparatus itself.

In summary, this modification of Gulliksen's tachistoscope allows the presentation of series of indefinite length of any sort of material which can be drawn or printed on paper cards. The presentation time and the interval between presentations can be independently varied through wide limits, and can be made as regular as the cycles of the A.C. supply or as irregular as desired. Merely closing a different switch changes the interval. Some of the inconveniences of the earlier models have been obviated by the substitution of thin cards for the heavier ones previously used.

As the exposure time can be made very brief the apparatus is adapted for use in work on span of attention or any other tachistoscopic experiment. Since longer exposures are obtained with equal ease, the apparatus is suitable for learning problems or experiments on abstraction and generalization.

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BOOKS

THREE THEORIES OF LEARNING*

"Psychology is today a more fundamental science than neurophysiology. By this I mean that the latter offers few principles from which we may predict or define the normal organization of behavior, whereas the study of psychological processes furnishes a mass of factual material to which the laws of nervous action in behavior must conform" (2, p. 24). This remark seems to me patently true and, in view of the recent history of psychology, profoundly important. But E. B. Holt does not believe it. For him, psychology must show "a responsible reckoning with the facts of physiology." (We shall see later that he means only certain selected facts.) The purpose of his book on *Animal Drive and the Learning Process* is, he tells us, "to present the outline of a non-faculty psychology in terms wholly of physical and physiological processes." For he is "convinced that these are the only terms in which we can find the solution of any of our psychological problems."

Although Holt makes it clear that he does not regard his elaborate theory as behaviorism, it is so evidently just what the latter would have become if it had lasted long enough to acquire a little physiological sophistication that it can hardly hope to avoid being so considered by others. But the label is unimportant. While it is impossible here to evaluate his argument in detail, a brief summary of his theory and a description of the method by which it is developed can be given.

There are no inherited dispositions. "No 'potential character' is ever 'already contained' in anything: and the notion of potentiality, wherever used, is a mark of finalistic thinking. The contents of the germ cell are not potential characters at all, whether bodily or mental: they are actual proteins and other substances, and to call these substances 'potential' this or that is to flout the truth. As Verlaine has said, there are no 'innate characters.' So that those persons who imagine that in the fertilized egg they are dealing with 'potential' or 'congenital' albinism, cyclopia, or lethal factor, with agoraphobia, herd instinct, or mathematical endowment, will in the end, of course, have to give place to more observant investigators who can recognize a carbohydrate or amino-acid when they see one. It is deplorable that such gross superstitions survive in modern biology

*Holt, E. B. Animal drive and the learning process: an essay toward radical empiricism: Vol. I. New York: Holt, 1931. Pp. xii+308.

Tolman, E. C. Purposive behavior in animals and men. New York: Century, 1932. Pp. xiv+463.

Thorndike, E. L. Human learning. New York: Century, 1931. Pp. 206.

and psychology" (p. 9). All that the contents of the germ cell can determine are the anabolic processes of the organism, the way in which its food materials are used. All katabolic processes are determined by stimulation from without. The author seems to forget that these processes are actually inseparable and that, even if they were not, anabolism constitutes precisely half of metabolism, thus leaving considerably more opportunity for intrinsic determination than he admits. "These enzymes . . . determine *how* this [food] shall be synthesized into higher organic compounds (anabolism) to make the *substance* of the organism. But this raw substance acquires ('learns') organic pattern by an inverse process, a katabolic one; stimulation from outside, axial gradients, organic axes, organs" (p. 18).

"Precisely because no connections between afferent, central, and motor neurones are 'preformed,' or established by 'heredity,' the earliest movements of organisms are utterly random movements" (p. 31). All the reflexes with which the animal is born have been learned during gestation; i.e., *all* reflexes are conditioned. The mechanism of conditioning is that implied in Kappers' description of neurobiotaxis: "dendrites grow towards an active neurone or nerve bundle provided that the neurone from which the dendrite grows and the neurone toward which it grows are in excitation simultaneously, or in close succession" (p. 26). "This dendrite growth will tend to reduce the extent of non-nervous, and more resistant, tissue across which subsequent nerve impulses arriving on the same 'to-be-conditioned' path must pass in order to reach the motor side (dendrites) of this synaptic region. . . . In my opinion this growth of dendrites under the stimulus of nerve impulses is the sole basis of learning" (p. 27). Most of the earlier reflexes are conditioned by means of the reflex-circle, whereby the contraction of a muscle is re-enforced and prolonged by the afferent impulses from the proprioceptors of the muscle itself, originally attracted by neurobiotaxis to the active path exciting the contraction. This is different from—indeed opposed to—Bok's reflex-circle which usually facilitates avoidant reactions. Holt's view is a necessary consequence of his neurobiotactic theory of conditioning, and he sticks to it and brushes the facts aside. All the early responses of the organism must, according to the theory be "*adient*" (of such sort as to get more of the stimulus): therefore, they *are* adient. How, then, can the putatively later avoidant responses be derived from these reflex-circles and prior adient responses? In more elementary terms, how can a muscle ever relax? By inhibition from its antagonist; but the only kind of inhibition compatible with the theory is the so-called Wedensky "inhibition by overcrowding." I leave it to physiological critics to pass upon the merits of a long (and very *ad hominem*) argument directed against Adrian's evidence against the Wedensky inhibition. It seems to be a case of the pot calling the kettle black.

Instincts are simply reflexes stimulated from inside. "The appetitive stimuli appear to be of two sorts, mechanical pressures and chemical

substances" (p. 126). "It is my belief that all of these internal reflex processes are *learned* from random beginnings, in ways strictly analogous to those we have studied; but they are learned during the foetal growth, when function and organ are developing hand in hand" (p. 130).

"Inasmuch as every afferent impulse spreads more or less widely as it traverses the central nervous system, the afferent impulse which is to be conditioned will somewhere come to a synaptic region which forms a part of the sensori-motor tract along which the 'unconditioned' impulses are travelling to a muscle. At this point of conjunction the conditions requisite for neurobiotactic growth are realized, and dendrites (on the motor side) of the synapse of junction will be stimulated to growth, contracurrently, toward the terminal arborizations of the neurone on which the to-be-conditioned impulses are arriving" (p. 27).

The method by which this system is developed is very simple and effective but not to be recommended as a scientific model: it consists in piling one dubious explanation upon another and ignoring the facts that do not fit. (Anything not manifestly impossible may be introduced as "very possible." Before long it has become quite probable, and by the time it is required as a premise for the next deduction it has attained almost the status of a fact.) Obviously, if one step in the argument is false, the whole subsequent structure must collapse: and nearly every step seems to be vulnerable. An example: if neurobiotaxis is the neural mechanism of conditioning, the unconditioned stimulus *must precede* the to-be-conditioned stimulus. Now Pavlov believes and states that conditioning cannot occur under these circumstances, but it has been demonstrated in two cases: the lid reflex in man and an escape response to strong electric shock in pigeons, both of which are what Pavlov would call "defence reflexes" and both of which have extremely short latencies. Holt cites these two cases and considers that they dispose of this objection to his theory of conditioning by neurobiotaxis. He gives two speculative "explanations" (pp. 87 f.) for the possibility of conditioning in the normal order, but does not attempt to show why this theoretically much more complex conditioning should be so much more universally and readily obtained. This latter fact alone would seem sufficient to exclude his neurobiotaxis theory of conditioning.

Holt does not always ignore inconvenient facts: sometimes he looks them in the face—and passes boldly on. Thus, though his theory requires that the earliest reactions be entirely adient, he cites the observations of Coghill, Paton, and others to opposite effect (p. 42). He is evidently aware of the work of Lashley and of P. Weiss, but not that their facts are completely incompatible with his theory.

Holt has a number of pet aversions and lays about him with great gusto, more gusto, indeed, than discrimination. Notable among these is "verbalism" or "word magic," from a variety of which he appears himself to suffer. Thus his concept of "adience," introduced as a consequence

of the reflex-circle and neurobiotaxis, soon becomes not "muscle-wise" but "limb-wise" and eventually "organism-wise"; and yet they are, by implication, the same thing! Either the use of this one term for such radically different things is an attempt to suggest an identity that is too manifestly absurd to avow, or he is "making a blunder with regard to transformation levels" (p. 83). Another example of word magic is the easy transition from reflex-circle adience to general sensory adience, e.g., visual. There is the usual confusion of "stimulus" and "object," an ever-recurring case of what Johnson aptly calls the "method of equivocance." There seems also to be some word magic in the term "impulse."

Holt also detests teleology and detects it everywhere: in Gestalt theory, even in Avenarius' "system C." It is too much to expect that he should detect it in himself, but it is there. Thus his "organism-wise adience" is no less teleological for being less specific than, say, McDougall's instincts.

One wonders sometimes what all the shooting is about, for we learn that, after all, mind is emergent. "As we proceed in this Essay we shall be able more and more to spare ourselves much of this neuro-muscular intricacy —so long as we are able to go back to it when we need to. For this mechanism produces as a synthetic result novel phenomena which we can deal with as such. Of this, adience of the organism, and avoidance, are two instances" (p. 109). "If from our strictly physiological start we do come upon mental phenomena, or perchance the general mechanism of the mind, it will be because we have reached a new 'emergent level,' that is, because nerves and muscles when organized in a more complex unity necessarily give rise to a new phenomenon which not at all resembles nerves and muscles; as happens so often in chemical syntheses, where two or more substances combine into a new compound which bears no resemblance to the ingredients that were combined. And in fact, the mind *is* precisely such an emergent; it is a new transformation level" (p. 256).

In view of this, one wonders just what the relevance to psychology of his elaborate and dubious physiological speculations may be. It is noteworthy that the farther he gets away from them the better his book becomes, and I anticipate that the second volume will be on a quite different level. There is some very good psychology in the chapter called "Motor Block," especially the description of distortion in a perceptual field by an obstructed need.

It is perhaps incumbent upon the reviewer to speculate a little as to how a psychologist of Holt's high quality came to write so bad a book. Many psychologists feel that they have to develop a—to them—satisfactory epistemology before they can work comfortably in psychology. In much the same conscientious way, Holt seems to have been under the additional necessity of constructing a—to him—satisfactory physiological basis for mind before going to work. Then, too, he seems to share the very general belief that the only philosophy sufficiently hard-boiled for science is ma-

terialism. And this book may be designed partly as a confession of that really very sentimental faith. As James pointed out, the genuinely positivistic and "tough-minded" philosophy is pluralism. At all events, Holt has provided an admirably consistent and explicit account of a set of beliefs implicit in much contemporary psychology. It will no longer be possible to dismiss criticism of such doctrines by remarking that the critic is belaboring a straw man of his own creation or that "no one ever maintained such a doctrine." That is in itself a useful thing, since it is not explicit but implicit fallacies that impede science.

In considering Tolman's book it is necessary to pay some attention to problems upon which the book itself is practically silent, especially to the problem of knowledge. For, though Tolman regards his system as a behaviorism, it seems evident that the only behavioristic thing about it is its epistemology. And this, though ultimately a mere lip service, has notable consequences for the entire treatment.

Thus behaviorism said, in essence, "We cannot know the minds or experiences of another, therefore we will ignore them or, better still, deny their existnce." In the same way physicists with a little philosophical sophistication might have said on equally good epistemological grounds, "We cannot know the physical world, therefore we will ignore it or, better still, deny its existence," and have turned to contemplation of their navels.

But in spite of epistemological difficulties—and these are in no wise dependent upon the transmission theories of light and sound which facilitated their recognition—physics has attained an impressive stature by trusting experience, mediated though it may be by such presumably fallible instruments as our bodies. Yet the same considerations that behaviorism urges against the possibility of a scientific psychology would make physics impossible. The point need not be labored since it has been admirably treated by Köhler in the first chapter of his *Gestalt Psychology*. Nor need we, as psychologists, be perturbed by differences in theories of knowledge. Probably all shades of opinion from naïve realism to idealism may be found among physicists. Thus one will say that electrons are real things and another that they are logical constructs, depending often on the field in which they work, but it doesn't really matter for physics. Now the data of psychology, perceivings, needs, goals, etc., are gotten by what may be called *automorphism*. Köhler prefers naïve, Lloyd Morgan critical, automorphism; and here again it is largely a matter of taste and of the particular problems in which one is most interested.

Thus if our interest is primarily in the dynamics of behavior we may say that we perceive an animal's need for an object or state of affairs quite as directly as we perceive the animal itself. That is naïve automorphism and it suffices for many psychological problems as naïve realism for large parts of physics. If, on the other hand, we are interested in,

say, ideation we may find ourselves driven to critical automorphism[1] in the same way that the worker in subatomic physics, if he think about it at all, finds himself driven to critical realism.

Tolman, however, is convinced that he avoids automorphism entirely by the device of using new and "objective" terms or by explicitly exorcising the "subjective" demons of old ones. And as this effort at verbal "objectivity" is the sole justification for the difficult terminology the reader gets a very vivid impression of the havoc wrought in a serious and admirable attempt at a systematic understanding of behavior by a superficial epistemological purism.

This effort to avoid what he contemptuously calls "mentalism" is, naturally, unsuccessful, for the only way in which such a scheme can be made to work even approximately is by ignoring nine-tenths of behavior, as behaviorism did. It is one of Tolman's outstanding merits that he does not do this. In the scope of his system and in his readiness to test it against any and all facts of behavior he compares with McDougall. But by the same token he cannot and does not remain a behaviorist. For, in spite of repeated assurances that there is nothing mentalistic in his signs, goals, expectations, etc., they continue to display properties which can be known only through his knowledge of his own experience. Even where the terms originally had no mentalistic significance (such as behavior-adjustment, commerce-with, manipulanda, etc.) their meaning depends ultimately upon his and his readers' knowledge of their own experiences.

Thus he says:

> "Whereas for Professor Perry and for us purpose is a purely objectively defined variable which is defined by the facts of trial and error and of resultant docility;[2] for Professor McDougall, purpose seems to be an introspectively defined subjective 'somewhat' which is a something other, and more than, the manner in which it appears in behavior; it is a 'psychic,' 'mentalistic' somewhat, behind such objective appearances, and to be known in the last analysis through introspection only. This difference between our point of view and Dr. McDougall's is fundamental and implies a *bouleversement complet*" (p. 16).

Now is seems to this reader, at least, that this difference with McDougall is only epistemologically fundamental and that in his psychological theory even this difference quite evaporates, for though he infers purpose originally from "objective" behavioral facts, he is quite as free as McDougall in attributing to it properties and agencies not so derived.

Tolman in his preface expressly anticipates a related sort of criticism

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[1]Cf. (1).

[2]His use of the term docility is that of R. B. Perry, Yerkes' *Introduction to Psychology* (4) and originally of Royce's *Outline of Psychology* (3). It may be described as "modifiability by consequences."

when he says: "Actually, I have used these terms *purpose* and *cognition,* and the various derivatives and synonyms I have coined, in a purely neutral and objective sense. Yet I know that, in spite of this fact, most readers will persist in believing that I mean by *purposes* and *cognitions* entities ultimately subjective in character and metaphysically teleological in import. I can but hope that all such readers will read and reread the glossary." He evidently fails to realize that these terms or any "objective" ones he may substitute for them can have a meaning for the reader, can be understood by him, only through a reference to his own experience. It is Tolman's privilege to make this reference obscure and difficult—and his psychological theory is good enough so that he can do that and still not alienate his audience—instead of clear and easy, but if he think that by so doing he eliminates the necessity of this reference, he is simply mistaken. If he reflects on the nature of language he will see that it is merely a peculiarly facile and fluid way of behaving alike in order to facilitate the automorphic apprehension of the experience of others. Thus ultimately the net result of this effort to lift himself by his epistemological bootstraps is to make the reading difficult. The reviewer's task might then seem to be to translate the theory into psychological terms, to see what is new in it and whether what is new is true. But even the first of these is manifestly impossible within the limits of a review.

Tolman has thoughtfully provided his book with a glossary of some eighteen pages defining his novel terms and this is a great help, though the hurried reader who thinks he can understand the system from the glossary alone will be grievously disappointed. Let us examine a few of these definitions and then essay a summary interpretation of the system.

> *Awareness, conscious.* The process of "sampling" by "running-back-and-forth" may be overt and with respect to environmental objects then and there actually sensorially present. In this case it is to be designated simple awareness. Or it may be in the form of mere behavior-adjustments and be relative to environmental objects not then and there sensorially present. In this latter case it is to be designated ideation.
>
> *Manipulanda.* Manipulanda are the characters of objects which support (see Behavior supports) motor activity (manipulations). They derive in character from the independent physical character of the environmental object and from the response-organ make-up of the given organism. They are such properties of environmental objects as lengths, widths, weights, resistances, solidities, fluidities, etc. But they are these properties defined not as such, and in themselves, but in terms of the range and refinements of manipulations which they will support in the given organism. They are stand-on-able-nesses, pick-up-able-nesses, sit-in-able-nesses, etc., etc.

In this definition there is clear appreciation of the necessity of dealing with what I have called the animal's field rather than with the physical

situation, i.e., usually, the experimenter's field. This I regard as one of the most important achievements of the entire book. One more definition:

> *Means-end-readiness.* A means-end-readiness (sign-gestalt-readiness) is one of the most important kinds of immanent determinant. It is a selective condition which an organism, due to innate endowment or past training, brings with him to specific concrete stimulus situations. It is set in action by virtue of a demand to get to or from some given type of goal object. It is equivalent to a "judgment" that commerce-with such and such a "type" of means-object should lead on by such and such direction-distance relations to some instance of the given demanded type of goal-object. It causes the organism to be responsive to stimuli and to perceive, to mnemonize, or to infer particular instances of such "ready-for" means-objects. That is, it is means-end-readinesses (sign-gestalt-readinesses) which determine the selective responsiveness of organisms to stimuli.
>
> It is the carrying over of such means-end-readiness from one situation to another which constitute (*sic!*) the so-called phenomenon of "transfer" of training.

Let us turn now to a brief summary (in translation!) of the theory, which is bound to be unjust and inadequate and which will probably prove to Professor Tolman that I have utterly mistaken his meaning and misconceived his intention.

Animals need ("demand") to get to some things or characters of things or to some states of affairs and to get away from others. This is due to appetites and aversions which are in turn due to (or *are?*) certain physico-chemical disequilibria, actual or threatened. The appetites (food, sex, excretion, specific contact, rest, and aesthetic hungers), are cyclical; the aversions (fright and pugnacity) are relatively continuous. Both are docile. Animals cognize or "expect" these ends or goals (demanded objects), their functional distances and directions, and such other relevant facts ("behavior supports") as barriers and means, by perception, mnemonization, and inference. Certain "behavior supports" are "demanded" as means (by virtue of "means-end-readinesses," judgmental in character because "docile") and thus become secondary or subordinate goals. There may be, and in most behavior there is, a whole hierarchy of such goals. Some goal objects satisfy the ultimate appetite involved better than do others (are better means) and the "demands" for them are conceived to be stronger. Behavior supports are of several varieties: "discriminanda" (sensations?), "manipulanda" (meanings?), and "means-end relations" (i.e., functional direction and distance). Means objects become "signs" of the end they serve and are organized with that end ("significate") and the means-end relation to form a "sign-gestalt." "A sign-gestalt once acquired may thereafter function as a single object." Insight ("inventive or creative ideation") is the inferential extrapolation of a new sign-gestalt.

Learning, then, as Tolman sees it, is the formation of sign-gestalts,

and in this he is beyond cavil correct. He gives an elaborate list of "laws" of learning which fall under two heads, those concerned with capacities and those concerned with the field. The latter are further divided into laws relative to interrelations in the material and laws relative to the manner of presentation. These laws are really statements of the conditions necessary or favorable to learning and they vary somewhat for what he regards as the three kinds of learning, namely, conditioned reflex, trial-and-error, and inventive learning. They do not tell us *how* this formation of sign-gestalts or this reorganization of a field comes about. But one cannot complain of that since no on else has done that. Indeed, it is only recently that we have begun to see the direction that research must take in order to answer this central question of the whole problem of learning. Tolman's laws, which I shall not here rehearse, are bound to be of value to any student of learning for their succinct summarization of what, from his point of view, are the necessary conditions of learning.

I must add that this point of view seems to me insufficiently *genotypic*, to use Lewin's striking term. Thus he seems to waver between the excellent conception of learning as a process, namely, the formation of sign-gestalts, and some more performance-defined conception of learning. The latter is especially evident in his criticism of the law of effect and in his discussion of what he calls "latent learning." He recapitulates his criticism of the law of effect as follows:

> "Let us sum up, now, as regards the Law of Effect. First it appears that 'latent learning' takes place perfectly successfully without any very appreciably greater 'effect' for what is later to be the correct response than for what is later to be the incorrect response. An animal acquires a capacity to differentiate between the 'right' and the 'wrong' responses during a period when the one response is only slightly, if any, 'righter' than the other. And the amount of this ability to differentiate, thus built up, seems to be quite as great in the end as that accomplished by control animals who, during the same period, are rewarded strongly for taking the one act and not for the other.
>
> "Second, it appears, at least in the case of human subjects, that when individuals are punished on the correct responses they may learn to make those correct responses more readily than a control group punished on the incorrect responses, because for some reason a 'bad' effect on the correct response seems to be more 'emphasizing' than a 'bad' effect upon the incorrect response" (p. 346).

Now if he had held to his conception of learning as the reorganization of a field or the formation of a new sign-gestalt it would have been evident that in spite of the identity of the end-results the weakly motivated animals in Blodgett's experiment had done their learning, i.e., their formation of a new sign-gestalt, only after the introduction of a more substantial goal. The whole notion of "latent learning" shows that he has been betrayed

by the universal performance conception of learning. His second criticism of the law of effect seems to me equally ill-taken for reasons which he himself half recognized. I am further at a loss to understand his attitude toward the law of effect since it seems to be the essence of his fundamental conception of docility.

The same wavering between his explicit sign-gestalt theory of learning and his implicit behavioristic one is evident in his treatment of the law of exercise. Here, after using to good effect the excellent experimental evidence provided by Thorndike against the law of exercise, he smuggles it into his own list of laws under the name of "frequency," and here again he is betrayed by the performance conception of learning and by his devotion to mazes.

There is an echo of functional psychology in the assertion that consciousness (also called awareness and simple awareness) equals "an actual running-back-and-forth" which occurs only in a choice or conflict situation. "Ideation equals an adjustment to such running-back-and-forth. It is a surrogate or substitute for such an actual running-back-and-forth and accomplishes the same end" (p. 211). These adjustments need involve no motor discharge whatever. Thus the Watsonian implicit vocal and skeletal response is attenuated to nothingness and Tolman's theory rightly goes back to Stricker's, originally published in 1880, with the addition of non-vocal adjustments to permit infra-human animals to think.

Sensations in the sense of "raw feels" (probably Titchener's 'attributes' or 'dimensions') are ineffable: only "discriminanda expectations" can be material for science. (It seems to the reviewer that von Hornbostel and his students are making excellent scientific progress in dealing with these "raw feels." Their experiments on the unity of the senses surely show that this can be done—if one doesn't worry too much about epistemology!)

Tolman's book more or less explicitly invites comparison with the theories of McDougall and Lewin, and this would be an attractive undertaking but much too long for the present purpose. Suffice it to say that it is quite near both, though not so near either as they are to each other, and it seems to me not quite so adequate as either. I suspect he will find it necessary to infer from behavior not only the functions he has already inferred, but also some enduring, organized—dare I call it "psychical"?—*structure*. Whether it be Lewin's soul with its psychical systems or McDougall's mind with its sentiments makes little difference—he will probably find some hyphenated new term that will take the "mentalistic" curse off it! Many of his present concepts have highly structural implications, e.g., the various readinesses.

The colloquial style of parts of the book, though at first perhaps ingratiating, may become annoying and, together with the neologisms and hyphens, may retard the system's getting the hearing it deserves. On the other hand, there is much vivid and precise characterization. The concise but usually

adequately descriptions of experiments are admirable, and their effectiveness is enhanced by the generous use of figures.

I have used the book as a text in comparative psychology, and if sufficient pains are taken to articulate it with psychology—with the baneful "mentalism" it combats—it can be recommended for the purpose. It has the tremendous advantage over a compendium that the experiments cited, though, to be sure, fewer in number and limited in scope, are cited in relation to a theory and not merely strung together with perhaps some such slender organizing principle as phylogenetic order. For relevance to a theory (even to a bad one—and Tolman's is not bad) is critically important not only scientifically but pedagogically.

Thorndike's book, *Human Learning,* is perhaps already too well known to require review but it will be of interest to note briefly the development his theory of learning has undergone. The book has an importance out of all proportion to its slight bulk. Some of the experimental data reported in it have been more fully reported elsewhere, notably, in the Washburn Commemorative Volume of the *American Journal of Psychology* and in the *Proceedings of the National Academy of Sciences.* A full technical account of others has recently appeared. The seriousness with which Thorndike takes his theory and the ingenuity shown in devising experiments to test it and all of its implications that he can think of make the book a valuable lesson in method. There is, especially in America, a deplorably general belief that research is research, that theory is theory, and a grim determination that never the twain shall meet. Consequently, we have theories that no one ever thinks of testing experimentally, and experiments that have no conceivable relevance to any theory. Naturally, both suffer. The elegance of Thorndike's experiments and their theoretical importance should do much to correct this state of affairs. This does not mean that the deduction of the implication tested is in every case correct or that the significance of the result is limited to that indicated. But in general his experiments prove what he says they prove.

The most important of these demonstrations are that of the irrelevance of mere frequency to learning and that of the importance of consequence. The former should put an end, once and for all, to the acceptance of repetition as a causal factor in learning, a result that the entirely adequate *a priori* disproof of it failed to achieve. The latter is equally important, but I believe that describing the observed phenomena in terms of Law of Effect confuses two or more separate functions. If learning be regarded, as Tolman, I believe, correctly regards it, as a reorganization of a field the Law of Effect has two different bearings. In the first place, so far as the reorganization itself is concerned it contains, I believe, the propositions that in all cases of learning there is a need and there is an obstacle to its satisfaction. In the second place, it states that the reorganization, once brought about, is in some way fixated or mnemonized by the resulting sat-

isfaction. Thus, the next time that the animal is placed in the particular situation he will have, not the field that he had in the beginning of his previous experience, but the reorganized field that he had at the conclusion of that experience. Thus, while the Law of Effect is an adequate description of the facts observed, it seems too phenotypic, too unanalyzed further to illuminate the problem of learning.

Thorndike calls his system connectionism appropriately enough but it is connectionism with a difference, a difference foreshadowed in his classical cat experiments where he found that an act was more readily learned when it had some sort of non-arbitrary relation, however obscure, to its issue. This difference he now calls the "principle of belonging." In this one expression he seems to have brought in a large part of Gestalt psychology for it is precisely the nature of this belonging of some things together that seems to be at least a very central problem of Gestalt psychology. Thorndike does not seem much concerned about the nature of belonging. For the purpose of his theory of learning it is sufficient that *some* kind of belonging be present, that is, not necessarily physically present but present for the individual involved. Thorndike seems much of the time, in the present work, to be dealing with fields instead of situations but they are still occasionally mixed.

The principle of *"identifiability"* seems to be that used by Gestalt psychology under the name "figure-ground" and he recognizes the important fact that learning "includes changes in the identifiability of situations as well as changes in connection leading from them to responses." His principle of *"availability"* seems to be the same thing in motor terms.

In spite of this progress in the cognitive aspects of his theory, the treatment of conation remains the same practical hedonism that he has made famous. His neglect of the dynamics of mind does not mean that he belittles it. "It is rather that the general importance of the latter (set, purpose, etc.) is obvious" (p. 121). Truly its *general* importance is obvious, but can a theory of learning long remain satisfactory that neglects the specific mechanisms by which needs, purposes, intentions, and goals operate?

He offers an hypothetical physiological basis for learning which will seem to many psychologists sufficiently damned by its calm neglect of Lashley's evidence against any specific path theory. While he does not satisfactorily absorb the conditioned reflex into his theory of learning he makes a very interesting suggestion:

> "It may be that learning does occur by mere sequence with or without belonging, very rapidly, if the stream of behavior is narrowed to a single string or series of sequences, and shut off from all the rest of the mind and brain so that there are no competing connections at any point. The conditioned stimulus, 1, the ordinary or unconditioned stimulus, 2, and the response, 3, may by the conditions of the experiment constitute something approximating such a shut-off, insulated system or series so that

> the brain process of 1 may lead on to that of 3 because that is the only course open to it."

It is difficult to evaluate Thorndike's present theory in any detail since he uses the term 'connection' in some eight different senses, statistical, psychological, neurological, etc., as he himself, with a candor as rare as it is engaging, points out. Doubtless he knows in every instance the precise one that he is using, but the reader would appreciate subscripts, at least. He is not troubled by epistemological or other philosophical niceties and that is clear gain. One has a feeling of feet-on-the-ground–common-sense and when he—in my opinion—errs, it is on that side, namely, on the side of the phenotypes or appearances, as in the case of the law of effect.

If, after examining separately three books devoted largely to the problem of learning, an effort should be made to characterize briefly their joint contribution, the following should be noted: Holt is largely preoccupied in the present volume, which it must be remembered is but half of the projected work, with a highly speculative physiological basis for learning, while Thorndike and Tolman give little attention to this. The latter are more acutely aware of the importance of organization to learning. All show increased recognition of the intimate interdependence of cognitive and conative factors in learning. That is, there is less of the tendency to regard what used to be called "motivation" as something necessary to but in some way apart from the learning process. This is clear gain. There is also increased recognition of the fact that psychology must deal with fields rather than with situations, and this is perhaps clearest in Tolman. On the whole, these three books represent a distinct advance in the attack on the learning process, not uniform, yet not without indications of convergence when the problem itself is properly conceived.

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E. D. Adrian. *The Mechanism of Nervous Action.* Philadelphia: Univ. Pa. Press, 1932. Pp. x+103.

It is often true that the length of a review is in inverse proportion to the merit of the book reviewed. From such a standpoint any review of

Professor Adrian's *The Mechanism of Nervous Action* must necessarily be short, for this is indeed a noteworthy contribution. Written in a lucid, conversational style, *The Mechanism of Nervous Action* gives to the reader an insight into the most recent developments in electrophysiology, and does it in a concisely informative manner. Within the limits of 94 pages Professor Adrian discusses such topics as the action of the sense-organs, the discharges in motor nerve fibers, and the processes taking place in nerve cells; most of the discussion is, of course, concerned with his own research and that of his co-workers.

The discussion on the action of the sense-organs should be of particular interest to many psychologists. In this section Adrian reviews the work by Cattell, Hoagland, and himself on tactile receptors and discusses the recent observations of Wever and Bray. In dealing with the topic of pain he suggests that the most typical pain responses seem to be correlated with fibers which have slow conduction characteristics and are joined to end-organs of relatively low excitability. An interesting feature of this discussion centers about the rôle of summation in the differentiation of such responses as touch and pain. Here Adrian seems to imply that the intense effects of pain may be due to a high degree of convergence in the pain pathways.

The last two chapters of the book deal with the problems of the discharges in motoneurons and the activity of nerve cells. The discussion on the motoneuron discharge is developed with respect to the question of the gradation of activity in muscle. In this Adrian cites the work of Denny-Brown and Bronk and himself in support of the position that gradation is brought about by concomitant variations in the frequency of discharge in a single unit and by a change in the number of units aroused to activity.

The last chapter, that on the activity of nerve cells, presents the greatest opportunity for interesting speculation. Here Adrian discusses the questions of the automatic rhythm of the respiratory center, the periodic discharges in injured nerve fibers and the synchronization of nerve cell potentials. One receives the notion that the study and explanation of such effects will contribute greatly to the understanding of central interaction processes, but as the final sentence reads: " . . . . . this is for future work to decide."

The Johnson Foundation is to be commended for its part in making available to a scientific public a book so important as this of Professor Adrian's. May all of the Lectures in the future be on the same high plane!

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